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THE PROBLEM OF ADAPTATION AS ILLUSTRATED BY THE FUR SEALS OF THE PRIBILOF ISLANDS.

BY GEORGE H. PARKER.

(*Read April 23, 1915.*)

The breeding habits of the Alaskan fur seals are so unusual as to make these animals unique among mammals. During much of the year, these seals are strictly pelagic roaming over the eastern expanse of the northern Pacific as far southward as the latitude of southern California. As summer approaches, practically the whole herd consisting of several hundreds of thousands of individuals repairs to the two small islands of St. George and St. Paul in Bering Sea for the breeding season. It is the relative proportions of the various constituents of the herd during this breeding period that affords material for interesting speculation.

The movements of the fur seals in their arrival and departure from the Pribilof Islands take place with much regularity. Early in May and June the mature males or bulls, having made their way through the passes of the Aleutians, reach the breeding beaches or rookeries on the islands of St. George and St. Paul. Here they take their positions, fighting all intruders while they await the coming of the females or cows. The cows arrive on the islands chiefly during June and July. They associate themselves with particular bulls and the bull with his group of cows constitutes the family unit or harem. In 1914 the average harem was not far from one bull

PROC. AMER. PHIL. SOC., LIV. 216 A, PRINTED JUNE 19, 1915.

to sixty cows, and the range extended from harems containing one cow to some that contained over a hundred. Because of the many years of commercial killing, chiefly directed against the males, it is impossible to state what the size of the normal average harem should be, but probably not far from one bull to thirty or forty cows.

Within a short time after the arrival of the cow, in the harem, *i. e.*, within a few days or a week or so, she gives birth to a single young or pup. So far as is known, cows do not produce more than one pup at a time. Shortly after the birth of her pup, the cow goes into heat, pairs with the bull, and becomes pregnant again. As these are annual occurrences, the period of gestation in the fur seal must be a few days less than a year. The pups are born males and females in about equal numbers. The counts of former years, as well as those of 1914, show a slight predominance of males, the excess being from a little over two per cent. to about seven per cent. of the total births.

The breeding season closes toward the end of July or early in August and this close is marked by the disintegration of the harems. During August most of the bulls begin their migrations back to the Pacific, and the pups, which heretofore have remained on the beaches, begin to take to the sea. They and the cows stay about the islands till November, when they too start on their migration to the open ocean. The only important constituent of the herd that has not yet been mentioned is the class known as the bachelors, *i. e.*, the young males that have not yet attained to breeding. The bachelors move with the cows, arriving for the most part in June and July, and departing in November, though some are found on the islands in December or even later. The bachelors do not mingle on the beaches with the rest of the herd, but gather to one side of the breeding grounds proper in the so-called bachelors hauling grounds, where they lead an idle rollicking existence suggested by their name.

The maximum age of the fur seal is believed to be about twelve to fourteen years for both males and females. In the migration, the males return to the islands approximately in the sequence of their ages; the old bulls arrive first in May and June followed by the younger bulls and bachelors and lastly by the yearling males, which arrive in the latter part of July and in August. The year-

ling males on arrival associate with the pups and cows rather than with the other bachelors. The bachelors may begin breeding at five years of age or even four, but they do not normally undertake this function until they are six or seven years old, when they desert the bachelors' hauling grounds for the breeding rookeries. The period of their normal breeding life covers, therefore, a term of perhaps some seven years or more.

It is not impossible that the yearling females do not return to the islands or, if they do, it is probable that they do so only in small numbers and late in the season. The two-year-old females return to the islands in July and August as virgin females, pair with the younger bulls, and reappear a year later, the end of their third year, with their first pup. From that time on they enter into the regular breeding of the herd and continue in all probability to produce one pup annually. Their breeding life, therefore, extends over some ten or more years.

These in brief are the main facts concerning the breeding habits of the Alaskan fur seal, an animal that exhibits one of the most remarkable examples of concentrated and localized breeding known. When it is recalled that these seals range over thousands of miles in the northern Pacific and that all sexually active members of the species without exception congregate in the appropriate season on the two small islands of St. George and St. Paul for breeding, the very exceptional nature of their reproductive activities must be evident.

The proportion of the two sexes at birth is very nearly equal, yet when the breeding age has been reached, the natural relations are not far from one male to thirty or forty females. As there is no reason to suppose that the death rate is higher in males than in females and as the length of the breeding life of the two sexes is not very different, about seven years for the bulls and about ten for the cows, it follows from the sexual proportions already mentioned, that we should expect an excess of bulls to be present. As a matter of fact, such is the case, for even in 1914, after the excessive commercial killing of males in the past, the so-called idle bulls were much in evidence. It thus appears that the Alaskan fur seal produces at birth approximately equal numbers of males and females

and yet in its breeding activities needs only relatively few males, a condition which when viewed as a whole seems to be a misadjustment rather than a close adaptation to the actual needs of the species. The measure of this misadjustment would be the proportion of idle bulls naturally present. Unfortunately, the commercial activities of the past in exploiting the herd for its fur prevent the possibility of accurate statement on this point, but the presence of idle bulls in the herd today is enough to show that this class under natural conditions would be abundantly represented.

The fur seal, however, is not the only one of the higher animals to show this misadjustment in the ratio of males to females. A prosaic example of the same kind is seen in the barn-yard fowl. Here the sexes hatch in nearly equal numbers, there being perhaps a slight predominance of females, but in maturity the cock holds sway over a flock of hens. This condition is almost exactly parallel with that of the fur seal except that it occurs under domestication. Nevertheless it has probably been inherited from the wild stock, for Finn states that though the red jungle fowl will live quite happily with a single hen, this is not universal and harems are often found. The bull of the American elk or wapiti, as my friend Dr. J. C. Phillips tells me, also forms, during the breeding season, a harem of cows from which he will drive away other bulls of his own kind, much as the fur seals do. Dr. Phillips further informs me that there are among the higher vertebrates many other instance of that particular form of polygamy in which one male during the breeding season naturally associates with many females. Such examples are found among some of the larger antelopes, wild sheep, and wild goats, and among certain birds such as the black grouse, capercaillie, and wild turkey. Although in these several species, the proportions of sexes at birth, so far as I am aware, are not definitely known, they probably follow the rule of approximate equality so common among many of the other higher animals and thus in reality illustrate much the same condition as that seen in the Alaskan fur seal.

Among the lower animals, particularly the insects, exceptional ratios in the sexes have long been known, the classic example of the honey bee being the most commonly quoted. Here a few

males, the drones, are set off against one perfect female, the queen, and a host of imperfect ones, the workers. These cases differ from those in the higher animals, however, in that the sex ratios appropriate for the breeding colony are determined from the beginning, *i. e.*, the young are not produced males and females in equal numbers. Such cases as the honey bee and other like insects exhibit, therefore, in their sex ratios much more accurate adjustments to their breeding requirements than do the higher animals; in fact they may be said to show a very high order of intracolonial sex adaptation.

Throughout the animal kingdom as a whole sexual reproduction seems to be best adjusted where the sexes are represented in approximately equal numbers and this relation is probably determined by the production of equal numbers of male-determining and female-determining sexual elements. The sperm cells of most species of animals, perhaps of all, are apparently the prime factors in this determination, and the dimorphism of these cells in the sense that one class is made up of male-determiners and the other of female-determiners as well as the production of these two classes in equal numbers may be looked upon as the chief adaptation of the animal kingdom so far as sex ratios are concerned. But the reproductive activities of a limited number of animals, such as the honey bee and the fur seal, have developed in directions in which equal numbers of the two sexes serve no longer as an advantageous combination. To meet these new conditions, further adaptation would be needed and, from what has been said, this adaptation would involve readjustments in the powers of the sex-determining reproductive cells. Such readjustments seem to have been carried out in the insects as seen in the honey bee, etc., where through the development of natural parthenogenesis the usual sex ratio has been entirely set aside and a new one favorable to the new requirements has been established. This has not been accomplished by the fur seals and other higher animals which in this respect remain poorly adapted to their new relations. From this standpoint, then, such lower animals as the insects show a higher order of adaptation than either the mammals or the birds. An explanation of this paradox may be found in the fact that the rate at which generation follows generation in in-

sects is enormous compared with that in the higher animals and further that geologically speaking the insects are much older than the mammals or birds. Hence they have had a much greater opportunity to adapt themselves to their conditions than has fallen to the lot of the higher animals. If the maladjustments of the sex ratios as exhibited by the fur seals and other higher animals are to be interpreted in the way indicated, it is clear that the evolutionary processes by which adaptation is brought about must often be slow and imperfect with the result that adaptation itself is better described, in the words of Bateson, as a poor fit than in the extravagant terms of eulogy with which many of the older writers clothed it.

HARVARD UNIVERSITY,
April 23, 1915.

THE LARGE FRUITED AMERICAN OAKS.

By WILLIAM TRELEASE, Sc.D., LL.D.

Plates I–III.

(*Read April 23, 1915.*)

When Alphonse de Candolle monographed the oaks of the world something over a generation ago,[1] he distinguished with a varietal name a form of our common white oak with small acorns some 8 × 14 mm., which Engelmann had sent him—the usual fruit of *Quercus alba* measuring about 14 × 18 mm. Those who have examined numerous specimens of our common red oak, *Q. rubra,* and its double, *Q. Schneckii,* have noted that they occur in forms varying in diameter of the acorn from about 10 to about 20 mm. The assemblage of forms clustering about the Californian *Q. chrysolepis,* the oldest of our existing types of oak, geologically, also show a comparable or even greater difference in the size of the fruit of what are otherwise held to be mere variants of a single species; and the polymorphic *Q. dumosa* presents a similar if less extended range of fruit size.[2]

The most surprising of our species in this respect is the bur oak, which joins to its great range in size a difference in fruit which is even more startling; for while the usual diameter of the acorns of this species is somewhere about 25 mm., and of the cup five or ten millimeters more, the acorns frequently measure 40 mm. in diameter with a cup fully 50 mm. across on the one hand, while on the other hand they may scarcely reach a diameter of 10 mm. Perhaps no oak presents so great a range of cup characters as this species does.[2] While the round or ovoid scaly-fringed form covering the acorn nearly or quite to its top is taken as the most typical and has given to the tree its common name of mossy or overcup oak, it is not un-

[1] *Quercus alba microcarpa* A. de Candolle, Prodromus, 16[2] 22. 1864.

[2] On these consult Sargent, Silva, vol. 8.

common to find a broad saucer-shape assumed with the fringe of slender scales either seemingly absent, because closely inflexed beside the acorn, or extended in its development over a considerable part of the outside of the cup; and it is even possible, as Professor Pieters has recently shown me by material collected about Ann Arbor, for the cup of the smaller type of fruit to be shallow and thin as in the post oak, and either delicately ciliate at top or entirely without a fringe even on a single tree.

Even the largest acorns produced by these or our other familiar oaks seem small when compared with those of some tropical species of *Quercus,* or of the related genus *Pasania.* On our own continent, where the true oaks extend from the far north into the high Andes of Colombia, these large-fruited species are of both the red— and white-oak groups,—the former in Guatemala and Chiapas, and the latter in the last-named state of Mexico and along the flanks of the eastern Sierre Madre range above Vera Cruz. In contrast with these, which may reach a diameter of 50 or even 60 mm., the smallest acorns, also Guatemalan and Mexican, and of the group of red oaks, scarcely measure 5 mm. in diameter.[3]

Some two years ago, while looking over a series of type photographs that I had made in the course of a systematic revision of the oaks of tropical America, Mr. Walter Swingle, of the National Department of Agriculture, expressed considerable interest in some of the east-Mexican large-fruited white oaks as affording a hopeful field for experimentation both in direct propagation and hybridization, with reference to our own tropical and subtropical regions, and Mr. David Fairchild, of the same government department, considered the matter of sufficient interest to undertake importations through the interest of Dr. C. A. Purpus, whose collections in the southern republic have done much of recent years to make its vegetable wealth known. The purpose of the present communica-

[3] *Quercus parviglans* n. nom.—*Q. microcarpa* Liebmann, Overs. Dansk. Vidensk. Selsk. Forhandl. 1854: 184.—Liebmann-Oersted, Chênes Amer. Trop. 26. *pl.* 6.—Not *Q. microcarpa* Lapeyrouse, Hist. Abr. Pl. Pyren. 582. 1813. Equally small are the racemed acorns of an as yet unpublished group of west- and south-Mexican species of the red oaks; and the east-Mexican white oak, *Q. glabrescens,* possesses an equally small-fruited variety.

tion is to give a connected account of these large-fruited species, because of this popular interest that they possess.

The first of the large-fruited tropical American species to be made known is *Quercus Skinneri*[4] collected by Hartweg at Quezaltenango, Guatemala, which Bentham noted and illustrated in 1841 and described the following year. Skinner's oak is a large tree with long-petioled, ovate, acute, rather blunt-based aristately toothed glabrous leaves about 5 × 9 cm., producing solitary or paired short-stalked fruit resembling that of our common red oak but on a much larger scale, the shallow cup 50 mm. in diameter and the short-ovoid acorn of about that length. It is a red oak with the usual characters of apical abortive ovules and tomentose interior of the shell, but the latter is thicker than usual and with the septa intruded into the kernel so as to make the latter somewhat three-lobed. Of recent years *Q. Skinneri* has been collected only by Cook and Griggs, at the Finca Sepacuite, Guatemala. A similar, if not the same, red oak, but with larger duller winter buds and lance-elliptical leaves as much as 20 cm. long, was collected at Chinantla, in the Mexican State of Oaxaca, by Liebmann in the fall of 1842, but of it nothing else is known: as with *Furcræa longæva,* the species seems to range extensively through the Cordillera.

Closely related to Skinner's oak is a species recently collected by Dr. Purpus in the Mexican state of Chiapas, the similar acorns of which may reach a diameter of over 35 mm., their very shallow cups, with thickened scales, sometimes measuring 45 mm. across. This, which differs markedly from *Quercus Skinneri* in having acutely lanceolate very short-stalked leaves about 5 × 12 cm., may be called *Q. chiapasensis.*[5] Like the other oaks here under consideration, it appears to become a tree of very large size.

The year following the full description of *Quercus Skinneri,* the Belgian botanists Martens and Galeotti described under the name

[4] *Quercus Skinneri* Bentham, Gard. Chron. 1841: 16. *f.*; Pl. Hartweg. 90. 1842.—Hooker, Icones Plant. 5. *pl. 402.*—Liebmann-Oersted, *l. c. pl. B, 3.*

[5] *Quercus chiapasensis* n. sp. Arbor grandis: foliis brevipetiolatis, acutis, lanceolatis, aristato-dentatis; fructu magna; cupula plana, glabra, crassa; glande semiglobosa, 35 mm. diametro. *Q. Skinneri* affinis.

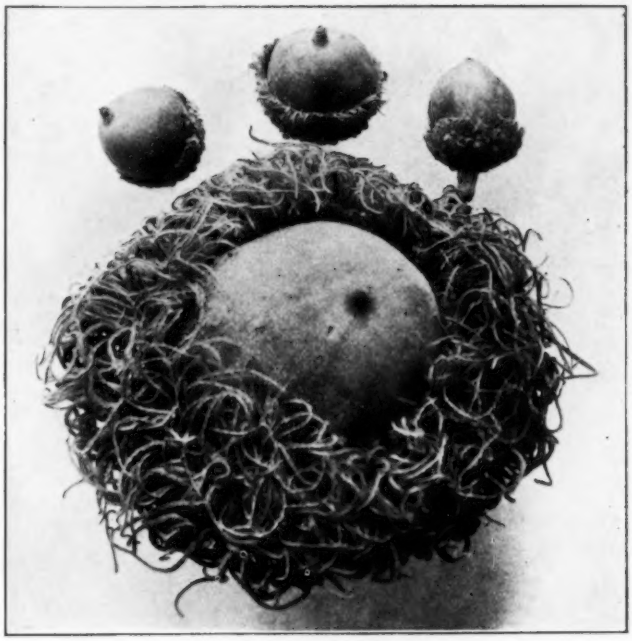
Q. insignis[6] what must be regarded as most notable in its genus because of the enormous height of the trees and the production of acorns occasionally fully 60 mm. in diameter and thus out of comparison with those of any black oaks and even with those of such Asiatic Pasanias as *P. cornea*. *Quercus insignis,* which occurs along the upper flanks of Mount Orizaba in eastern Mexico, is a white-oak, with the interior of the acorn shell not woolly, and with deeply lateral ovules. Its short-petioled elliptical-oblanceolate more or less acuminate leaves are sharply low-serrate but without bristle-like tips to the teeth, and measure 5 × 10 cm. or more. The fruit, which matures the first season, as seems to be true of all white oaks, is typically biscuit-shaped and about one-fourth shorter than thick, and the rather shallow cup, which may reach 80 mm. in diameter, is covered with coarse heavy loosely ascending scales.

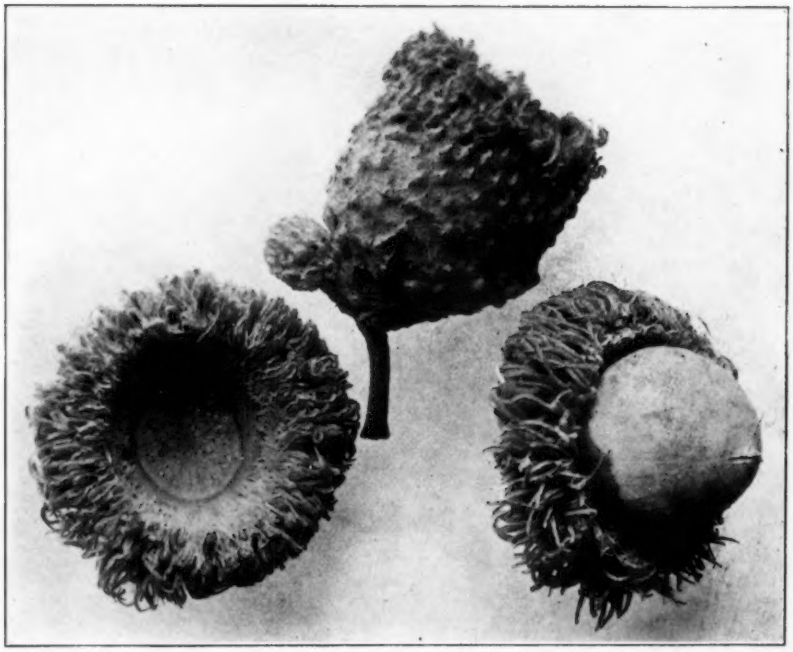
Martens and Galeotti do not appear to have seen more than one form in the oaks of this kind; but the type collection of Galeotti as represented in the museum at Budapest contains acorns of two sorts, one depressed, and the other acuminately conical and about as long as thick. At about the same time that Galeotti's collections were made, the Danish botanist Liebmann collected in the same general region materials of an oak similar to *Q. insignis* but differing in bearing subconical acorns about as long as broad and with more turbinate cups. This was published by its discoverer in 1854 under the name *Q. strombocarpa.*[7] Subsequently when a series of exquisite drawings of Mexican oaks prepared under Liebmann's direction were published by Oersted, the latter added a plate of a very similar form, which he called *Q. insignis strombocarpoides.*[8] It is hard to see how the latter can be distinguished from *Q. strombocarpa,* and the Galeotti collection shows that, different as the fruit extremes are, the discoverer of *Q. insignis* did not separate from its typical form the conical-fruited form to which apparently both of the later names refer.

[6] *Quercus insignis* Martens and Galeotti, Bull. Acad. Brux. 10²: 219. 1843. —Liebmann-Oersted, *l. c. pl. K, 29.*

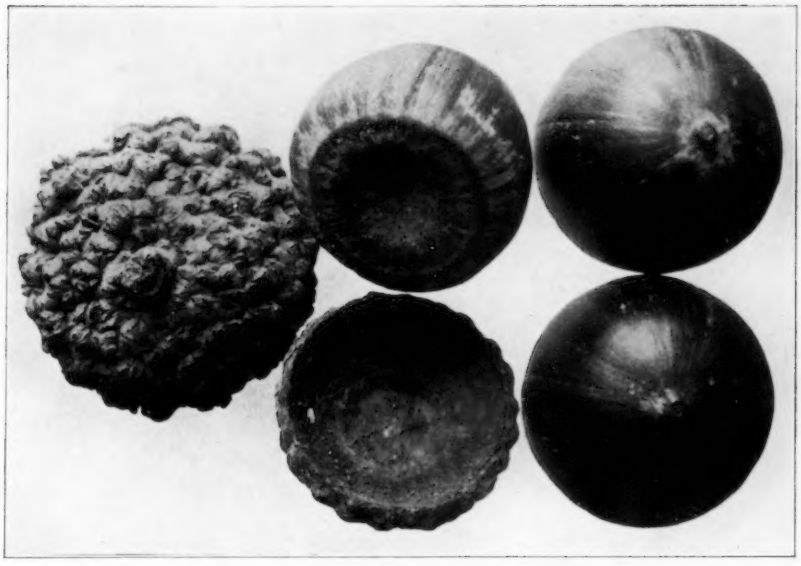
[7] *Quercus strombocarpa* Liebmann, Overs. Dansk. Vidensk. Selsk. Forhandl. 1854: 176.—Liebmann-Oersted, *l. c.* 24. *pl. 27.*—Oersted, Bidrag Kundsk. Egefamilien. 346. *f. E.*

[8] *Q. insignis strombocarpoides* Liebmann-Oersted, *l. c. pl. 28.*

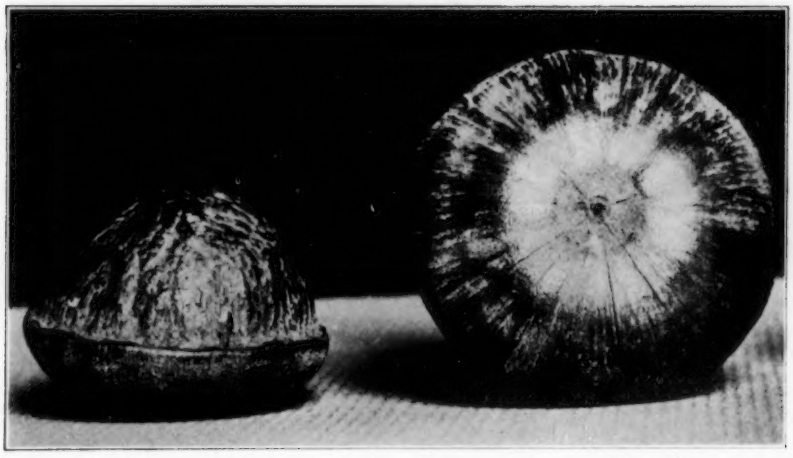




QUERCUS MACROCARPA.

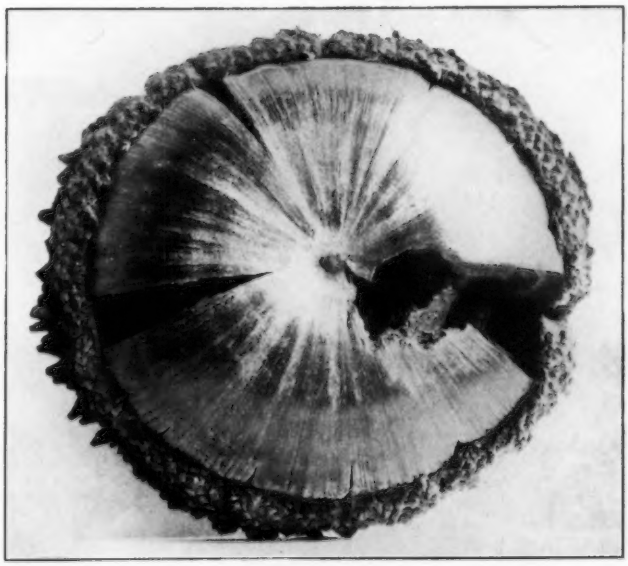


QUERCUS CHIAPASENSIS.



PASANIA CORNEA.

QUERCUS CYCLOBALANOIDES.



QUERCUS INSIGNIS.

Wishing materials that should throw light on this question, I turned in 1912 to my amiable correspondent, Dr. Purpus, who was then in eastern Mexico: but before my letter reached him, Dr. Purpus had gone into southern Mexico. The result of my appeal, therefore, was not further specimens of *Quercus insignis,* but collections of an equally large and almost equally large-fruited white oak which appears to be characteristic of the Chiapas region. With somewhat similar but more deeply toothed leaves about 7 × 20 cm., equally short-petioled, this combines a stoutly stalked turbinate cup as much as 60 mm. in diameter, the scales of which are barely if at all free at tip with their bases connate in zones; and the ovoid pointed acorn measures 40–50 × 50–60 mm. Though closely related to *Q. insignis,* this species is so distinct in its collective characters as to stand as the type of a separable group of white oaks, and it has been called *Q. cyclobalanoides*[9] because of its very peculiar cup-markings.

EXPLANATION OF PLATES.

(All figures are of natural size.)

Plate I.

Quercus macrocarpa. Above, three acorns from a very small-fruited Michigan tree (*Pieters*), partly with and partly without fringe to the cup, and a single fruit of the largest and "mossiest" type from Illinois (*Adams*). Below, two of the more typical acorns of different cup-depth, and a cup showing a not infrequent inrolling of the inner scales, also from Illinois (*Adams*).

Plate II.

Above, two cups and three acorns of the large-fruited Mexican red oak, *Quercus chiapasensis* (*Purpus*). Below, basal and side view of two acorns of the large-fruited Chinese oak, *Pasania cornea* (after photographs by Fairchild).

Plate III.

Above, two fruits of the Mexican ring-scaled white oak, *Quercus cyclobalanoides* (Chiapas, *Purpus*). Below, a fruit of the great-acorned Mexican white oak, *Quercus insignis* (Huatusco, *Purpus*).

The University of Illinois,
March 8, 1915.

[9] *Quercus cyclobalanoides* n. sp. Arbor grandis; foliis brevipetiolatis, acutis, oblanceolatis, mucronato-dentatis: fructu magna; cupula turbinata, luteo-tomentosa, pluriannulata; glande ellipsoidea, sub 50 mm. diametro. *Quercus* Insignes valde affinis, sed sectio distincta, Cyclobalanoideæ, constituens.—*Q. insignis* Journal of Heredity, 5: 407. *f. 12.* 1914—not Martens and Galleotti, *l. c.*

THE SWEDES, GOVERNOR PRINTZ AND THE BEGINNING OF PENNSYLVANIA.

By THOMAS WILLING BALCH.

(*Read March 5, 1915.*)

Of the original thirteen States, those south of the Middle States as well as those known under the collective name of New England, were settled by men and women of English race. New York, New Jersey and Delaware were first settled by Hollanders. The whole area of the Dutch settlements was known as New Netherland, and the chief city of the Hollanders in the new world was called Amsterdam in New Netherland, though historians afterwards thought fit to change the name into *New* Amsterdam,[1] doubtless because the English had renamed the town *New* York. The settlements in the valley of the Hudson and in what is now New Jersey passed by conquest into the hands of the English. The Dutch settlement in Delaware was destroyed after six months by the Indians. Subsequently, the Swedes took over the inchoate title of the Dutch to present-day Delaware. The Swedes later lost Delaware to the Dutch by conquest, who in their turn were afterwards conquered by the English.

No European Power, however, occupied and took possession of what today constitutes the Commonwealth of Pennsylvania, until Lieutenant-Colonel John Printz, who was the fourth Governor of New Sweden, moved up from Delaware to Great Tinicum Island and there established, in 1643, his seat of government, the first capital placed in the territory of the present State of Pennsylvania. He thereby became the first governor of the territory now known as Pennsylvania.

That Sweden was the first European nation to possess itself of what is present-day Pennsylvania was supported by the International

[1] I have to thank Mr. Robert H. Kelby, the learned librarian of the New York Historical Society, for this information.

Law of the seventeenth century. Towards the end of the sixteenth century there grew up as a rule of international law that, in order that a member of the family of nations could claim as its own a newly discovered and virgin land, it was necessary for that nation to actually occupy and possess that virgin land. The act of merely discovering and christening such an unoccupied land did not give the right of possession. The act of possession must be an actual occupancy through the establishment of forts and settlements in that land. Queen Elizabeth enunciated this principle clearly in 1580 in a notable answer she made at her court to the Spanish Ambassador, Mendoza.[2] It was thus recognized by England through the lips of her sovereign, a sovereign who well knew how to maintain the dignity and interests of her realm abroad. That rule became more and more recognized both by the publicists in their writings and by the nations in their acts, and has remained a rule of international law until the present day.

The sovereignty of Sweden over the land now known as Pennsylvania passed later by conquest to the States General of the United Netherlands, and subsequently again by conquest to the British crown, by whom it was afterwards granted to William Penn.

The fact that the sovereignty of Pennsylvania, alone of the original thirteen, goes back to Sweden for its beginning and that Printz was the first in the line of its governors, is known to only a very few. It would seem well then, that proper monuments to Printz and his Swedish settlement should be erected, so that future generations may know of the beginning of this province and state. And no place would seem more appropriate than the ancient hall of this venerable society of learning, the oldest existing society of learning not only within the bounds of Pennsylvania but also in all of the new world as well, to suggest that, first a bronze tablet should be erected in memory of Governor Printz and his capital called Nya Göteborg on Great Tinicum Island; and second, a bronze statue of Governor Printz, either of life or heroic size, should be placed at some conspicuous place in the city of Philadelphia.

[2] Camden's "Annals," 1580; see translation in Sir Travers Twiss's "Oregon Question."

GENERAL RESULTS OF THE WORK IN ATMOSPHERIC ELECTRICITY ABOARD THE CARNEGIE, 1909–1914.

By L. A. BAUER.

(*Read April 24, 1915.*)

Notable progress, it is believed, has been achieved by the department of terrestrial magnetism of the Carnegie Institution of Washington during the past year in the perfection of the instrumental appliances for observations in atmospheric electricity. In various articles by Drs. Swann and Hewlett, which have appeared in the *Journal of Terrestrial Magnetism and Atmospheric Electricity*, 1913–1914, new points of theory were brought out, serious errors in certain instruments were made known, and improved methods and instruments were devised. As a result considerable improvement has been made in the work in atmospheric electricity aboard the *Carnegie*, especially on her present cruise.

It is now deemed worth while to expand the work of the department in atmospheric electricity in two directions: (*a*) Continuous observations, by self-recording means, at the department's laboratory in Washington and at such observatories elsewhere as the department may be able to establish in the near future. (*b*) A general electric survey of the globe, implying observations at points distributed over the earth's surface, somewhat as in a magnetic survey.

Probably the late Professor Rowland was one of the first, in his address before the Congress of Electricians, held at Paris, September, 1881, to point out the need in atmospheric electricity "of a series of general and accurate experiments performed simultaneously on a portion of the earth's surface as extended as possible."[1] He says that "the principal aim of scientific investigation is to be able to understand more completely the laws of nature, and we generally succeed in doing this by bringing together observation and theory."

[1] Physical Papers of Henry A. Rowland, Baltimore, 1902, p. 212 et seq.

On Professor Rowland's motion the Congress resolved "that an international commission be charged with determining the precise methods of observation for atmospheric electricity, in order to generalize this study on the surface of the globe."

Unfortunately, in the past, the observations in atmospheric electricity have often been found to be counterfeits of nature because of the errors inherent in the instruments and methods used. Accordingly the much-desired discovery of nature's laws by "bringing together observation and theory" has not been effected in the measure desired. None of the proposals for a general electric survey of the earth which have been made repeatedly to learned academies, one of the last having been presented to the International Association of Academies, has been put into effect, doubtless because of the discouraging experiences encountered.

In spite of the vast work already done by notable investigators, we still have no generally accepted theory of the origin of atmospheric electricity.

Probably one of the most important of recent contributions to the observational data is the series of observations obtained on the past cruises of the *Galilee* and the *Carnegie*. A report giving the results up to the end of 1913, obtained by the department observers and others, was prepared by Dr. Hewlett and published in the September, 1914, issue of *Terrestrial Magnetism and Atmospheric Electricity*. The observations comprised, in addition to the usual meteorological measurements, those of the potential gradient, atmospheric conductivity and radioactive content of the atmosphere. Perhaps the most important result was a confirmation of the somewhat striking phenomenon, that while the conductivity over the ocean is, on the average, at least as great as over land, the radioactive content is much smaller. The values of the potential gradient obtained at sea were of the same order of magnitude as those on land.

Dr. Swann has just completed a report on the atmospheric-electric observations taken during the third cruise of the *Carnegie* while under the command of Mr. J. T. Ault, in 1914. The general course of the *Carnegie* during this cruise was as follows: Leaving Brooklyn on June 8, 1914, she arrived at Hammerfest on July 3.

Sailing again from Hammerfest on July 25 she entered the harbor of Reykjavik, Iceland, on August 24, having reached the latitude of 79° 52′ North, off the northwest coast of Spitzbergen. Leaving Reykjavik on September 15, the *Carnegie* arrived at Greenport, Long Island, on October 12, returning to Brooklyn on October 21, 1914.

The observations in 1914, comprised, in addition to the magnetic and meterological data, measurements of the potential-gradient, the conductivities for the positive and negative ions, and the radioactive content. Measurements of the ionic numbers were also made during the passage from Greenport, through Long Island Sound to New York. The whole of the observations, with the exception of a few measurements in Long Island Sound by Dr. Swann, were taken by Observer H. F. Johnston.

The average values of the potential-gradient, atmospheric conductivity, and radioactive content for the whole cruise were, respectively, 93 volts per meter, 2.52×10^{-4} E. S. U., and 23, the last number being expressed in Elster and Geitel units. The average value of the earth-air current for the whole cruise was 7.7×10^{-7} E. S. U. per sq. cm.

The atmospheric-electric elements were measured daily between the hours of 9 A. M. and 12 noon. The observations as far as they go indicate a general increase of the potential-gradient from summer to winter, which is in accord with land observations for the daily mean values. The conductivity also shows a general increase from the beginning of the cruise (June 8, 1914) to about the end of September, when a maximum occurs, after which the conductivity falls.

No marked variation of the atmospheric-electric elements with temperature or humidity was found. However, an indication is shown of a variation of the conductivity with latitude; a maximum for the latitudes involved occurring in the neighborhood of 50° North. These conclusions with regard to the variation of the elements with season, latitude, etc., must be looked upon as tentative owing to the small number of data involved.

The conductivity appears to have an especially low value in the neighborhood of the American coast. In Long Island Sound,

measurements were made of the ionic numbers n_+ and n_-, and the results indicate that the low values of the conductivity referred to above are to be attributed partly to a low value of the specific velocities of the ions (v_+ and v_-). The mean values of v_+ and v_- for observations on three days in Long Island Sound are respectively 0.77 and 0.83 cm./sec. per volt per cm. The average value of n_+ and n_- for observations taken on three days in Long Island Sound are 340 and 280 ions per c.c. respectively.

By making use of the value (23) given above for the radioactive content, and of the empirical relation obtained by Kurz, for the reduction of the Elster and Geitel unit to absolute value, it turns out that the average radioactive content for the whole cruise amounts to about 12 curies of radium emanation per cubic meter as against 80 curies per cubic meter which is about the average value found over land. The emanation content is thus too small to account for the conductivity observed over the sea, which conductivity is as great or greater than that measured over land.

A criticism of the ordinary method of drawing conclusions as to the nature of the radioactive products in the atmosphere, by comparing the decay curve with one obtained by a wire exposed in a closed vessel, is given in Dr. Swann's report. The activity curves are analyzed in the report mathematically, use being made of the theory of radioactive disintegration, and it is found that while some of the curves can be explained by radium emanation alone, others require the presence of a product of longer decay period than radium *A*, *B* or *C*. The possibility of this extra product being a product of thorium emanation, as is generally assumed to be the case on land, is discussed by Dr. Swann.

An attempt to calculate the actual amount of radium emanation in the air directly from the theory of the Elster and Geitel method, without assuming any empirical relation results in a much smaller value for the radium-emanation content than that given by the empirical relation unless it is assumed that the average specific velocities of the active carriers are much smaller than is generally supposed.

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THE RIGHTS AND DUTIES OF NEUTRALIZED TERRITORY.

By CHARLEMAGNE TOWER.

(*Read April 23, 1915.*)

Although the growing importance of the United States and their extended influence as a world power have made the subject one of prime interest to them in many respects heretofore, there has probably never been a time when the principle of neutrality has had for us in America the same weighty consideration that it has under the existing circumstances in the world today.

Never, probably, have the rights and duties of neutrals been so carefully scrutinized by American public opinion, or so sensitively tested by the responsible authorities of our Government. And very justly so, because, with almost the whole of Europe inflamed before us in this great war, there is scarcely a day in which some serious question does not present itself in the maintenance of our public policy, some delicate situation which affects our national honor,—both in our character of neutrals and our relations with the belligerent powers, and in their dealings with us in return.

It may be of interest, therefore, to consider one or two of the underlying principles of the rights and duties of neutral nations; not the less so, perhaps, because of the fact that neutrality, in its present recognized form, is the most recent and most modern of the effective rules of international law.

Indeed, the nations of antiquity had not only no conception of what we call neutrality, but they had not even a name by which to convey our meaning of the term. The Romans alluded to those not engaged in the war as *medii, amici* or *pacati;* and their dealings with them were regulated, as far as we can judge, by the feeling that they were peaceful and friendly; at all events that they were not openly to be regarded as the enemy. And this appears to have been the view of their position throughout the Middle Ages. It

was only in the seventeenth century that the term *neutralis*,—meaning to the minds of the people of that day, *non hostis*,—was brought into general use by the publicists, and that since then the condition of the neutral has been established, somewhat artificially, as is considered by some writers,[1] under the process of which the term *neutral* has been extended to the flag of a nation which chooses to take no part in the war, to its ships, its commerce and its citizens.

From this point of view it has been declared that neutrality is "the continuation of a previously existing state." That is to say: Powers which go to war and become belligerents alter their condition,—whilst those which choose to be neutral remain as they were before. Consequently, in their case, their international rights are unchanged; and "neutral states and their citizens are free to do in time of war between other states what they were free to do in time of peace."[2]

But, under the rules of international law, the state of neutrality carries with it certain rights and obligations which do not exist when there is no war. It has been settled that neutral governments may regulate the furnishing of certain articles to belligerent cruisers that seek hospitality in their ports, though they are bound to prohibit the supply of certain other articles, as, for instance, arms and ammunition. They have the right to enforce the respect for the neutrality of their waters, though they must not allow their territory to be used for fitting out or equipping armed expeditions against any belligerent. So also, the commerce of neutral individuals is subject to certain restrictions, as, in the matter of contraband of war, which do not exist in time of peace.

But the theory of the law is that these are merely the changes in certain details produced by common consent of the nations,—by the condition of war; though the principle remains permanently fixed, that the rights of a neutral continue, uninterrupted, in time of war precisely as in time of peace,—his rights of trade and commerce, his rights of free intercourse with either belligerent, or with anyone else; and that every restriction upon these activities that

[1] Holland, *Fortnightly Review*, July, 1883.

[2] Lawrence, "International Law," par. 243.

are lawful in a state of general peace must be based upon a clear and unquestioned rule of international law; the burden of proof being upon him who seeks to enforce the restraint.

As a general statement, the obligations of all neutral states are the same, so also are their rights, as non-belligerents and non-participants in the war; they decide by their own motion to occupy a neutral position, aside from and between the belligerents, with all of whom they voluntarily remain at peace. This is called "perfect neutrality," and is accepted by all the powers. But there are two classes of neutrals into which the whole body of neutral nations is divided, whose relations to the war are different in this respect: that, one set of them abstain by their own free will from entering the war; whilst the others are restrained from taking part in the hostilities and are obliged to remain out of it by the conditions of their existence. This difference between them marks the difference between neutrality and neutralization; between neutral and neutralized territory. And it is to this latter that I beg leave for a moment to direct attention.

A neutralized state, then, is one which is and must remain neutral under all circumstances. Its independent existence rests upon that condition. It is a state which has been constituted by common consent of the great powers, which has received from the powers the right to subsist, provided that it take no part whatever in any conflict that may arise between its neighbors and shall have no right of its own to take up arms except to repel attack or to defend its territory. Thus a neutralized state is, in fact, allowed to exist because the operative forces of self-interest of its neighbors find sufficient benefit accruing to themselves,—as, for instance, that it forms an intervening space between themselves and their own powerful neighbors whose proximity threatens their peace,—to induce them to agree to its existence. There are neutralized states, under international law, and neutralized individuals; and this character may be extended also to seas and waterways, to buildings, ambulances and ships.

A distinguished authority (Professor Holland) defines the process of neutralization as "the bestowing by convention of a neutral character upon states, persons and things which might otherwise

bear a belligerent character." But, "so great a change in their legal position cannot be made without the consent of all the parties affected by it. It must be made as the result of international agreement, in order to be valid, and must be accepted by all the important states."[3]

Neutralized states, therefore, are those which, whilst remaining politically independent, have yielded up a part of their sovereignty as the price of their existence, and are dependent upon the powers to protect them,—though they do not belong to the councils of the great powers, nor have they the right to discuss questions of policy which may ultimately lead to the employment of force, except in defence of their own frontiers.

The two conspicuous examples of this kind are Switzerland and Belgium. The cases are similar; each forms with its intervening territory a barrier between the threatened conflicts of powerful neighbors. Switzerland, lying as it does, between Germany, Italy and France, is so situated that if the passage through its territory were open, the Austrians might proceed freely from the valley of the Danube to the Rhone and the Po, and menace the western boundary of France throughout its entire length; and, indeed, that is what happened during the French Revolution, when the neutrality of Switzerland was disregarded and her territory invaded by all the contending parties, whilst the French, Austrians and Russians used her soil for their hostilities against each other. Again, in 1813, the Austrian army passed through Switzerland and crossed the Rhine at three places, in its campaign against France.

A short time later, the perpetual neutrality of Switzerland was recognized by the Congress of Vienna, in 1815; but, upon the return of Napoleon from Elba, the Allies called upon the Swiss Confederation to join in the general coalition against France, in order to assist them in promoting the common welfare of Europe and prevent the reestablishment of the revolutionary authority in France. They declared that they knew the importance attached by Switzerland to the maintenance of the principle of her authority, and that they did not intend to violate that principle; but with the view of accelerating the time when it might be made permanent

[3] Lawrence, ubi supra, paragr. 245.

and advantageous, they called upon the Swiss to assume an attitude and to take such measures as might be in proportion to the extraordinary circumstances of the moment, without forming a rule in this respect for the future. That is to say, the allied forces claimed the right to pass through Switzerland, recognizing her neutrality but agreeing that if it were violated by them they should not regard their act as a rule in the future. In truth, her neutrality was violated during the war by the contending parties on both sides.

But, after the reëstablishment of the general peace in Europe, a declaration was finally made, at Paris, in 1815, which fixed the political status of the Swiss Confederation, and upon that foundation it has rested ever since. By that declaration, both France on the one side and the allies on the other, Great Britain, Austria, Prussia and Russia, formally recognized the perpetual neutrality of Switzerland and guaranteed the integrity and inviolability of her territory. They declared also that the neutrality of Switzerland, and her independence of all foreign influence, were conformable to the true interests of the policy of all Europe.

The situation of Belgium renders it in this respect similar geographically to that of Switzerland; for it is the barrier which lies interposed between Holland and Germany on the one side and France on the other, and by means of its territory the boundary lines of these great powers are separated from each other in such a manner as to remove the menace of irritation which is always present in Europe where the common frontier is marked by a single line. With this barrier maintained, also, both France and Germany are protected from immediate attack at several of the most vulnerable points in the territory of each; as has been made evident by the conflicts that have taken place between the rival powers on the continent for hundreds of years, which have made Flanders and the low countries the battleground of Europe.

The territory of the present kingdom of Belgium was incorporated with that of Holland, in 1815, by the Congress of Vienna, in order to form the kingdom of the Netherlands, and for the distinct purpose of placing a barrier between the territories of Germany and France. But, quarrels of a domestic character having

broken out in the low countries, Belgium separated itself from the kingdom of the Netherlands, in 1831, the outcome of which was that a treaty was made, on the 19th of April, 1839, establishing peace between Belgium, as an independent kingdom, and Holland; and, on the same date, in 1839, another treaty was entered into by Great Britain, Austria, France, Prussia and Russia with the king of the Netherlands, recognizing that the union between Holland and Belgium, in virtue of the Treaty of 1815, is dissolved, and that Belgium, which is to be composed of certain provinces specifically delimited and set forth, shall become an independent state.[4]

This, then, is the origin and constitution of the kingdom of Belgium as we know it today. The powers agreed that, within certain boundary lines, it should be allowed to exist as a separate kingdom. They went further than that, and agreed also, by Article VII. of that Treaty, that:

We have in this a well-defined example of neutralized territory, as we are considering it today. Belgium was granted all the privileges of independence, with the right to make her own laws, regulate her own domestic affairs and administer her own government; always provided, however, that she should maintain, in her foreign relations, the strictest neutrality toward all other states. And this, it is believed, she has faithfully performed.

But, it will be observed that, whilst Belgium is thus bound to the great powers as to her neutrality, there is no agreement for specific performance upon their part in this respect, beyond their ratification of the convention itself and their general undertaking to carry out all of its provisions, in which the powers themselves had not entire confidence. It was evidently not regarded by them as a sufficient safeguard in the event of war, for when Germany and France declared war upon each other, in 1870, there was such grave danger that both the independence and the neutrality of Belgium would be disregarded in the course of the conflict, that it was considered necessary to assure her safety by special agreement having regard to the circumstances of that time.

[4] Hertslet, "The Map of Europe by Treaty," II., p. 984.

"Belgium, within the limits specified, shall form an independent and perpetually neutral state. It shall be bound to observe such neutrality towards all other states."

Therefore, Great Britain entered into a separate treaty with Prussia, in August, 1870, by which it was agreed that:[5]

"If during the hostilities the armies of France should violate the neutrality of Belgium, Great Britain would be prepared to coöperate with Prussia for the defence of the same in such manner as may be mutually agreed upon, employing for that purpose her naval and military forces to insure its observance, and to maintain, in conjunction with Prussia, the independence and neutrality of Belgium."

And Great Britain entered into a separate treaty with France, at the same time, making provision in the same terms for the coöperation with her for the defence of Belgium in case that Belgian territory should be invaded by the armies of Prussia. These separate treaties were made binding in each case upon the parties during the continuance of the War of 1870, and for twelve months after the ratification of the treaty of peace. Thus Belgium was protected against invasion or disturbance during the Franco-Prussian War; though since that time both her independence and her neutrality depend upon the old agreement between the five powers, made in 1839.

But, as an old French writer has well said: "With such neighbors there is always a chance for trouble." The unfortunate situation of Belgium leaves her always open to danger when her powerful neighbors begin to fight over her head. She has her defence in the old agreement of the powers, it is true. But will that be a sufficient defence when either or all of the powers, engaged in a desperate conflict amongst themselves, find that their own self-interest, then of prime importance to each of them, places the consideration of Belgium in the background? Evidently not; and in this respect all the powers appear to be alike.

For instance, Sir Edward Grey in his great speech in Parliament, on the 3d of August, 1914, whilst advocating the neutrality of Belgium in the present war, pointed to the *interests* of Great Britain as the determining factor in the observance of the guarantee entered into by the powers, in 1839.[6] He quoted to the House the speech which Mr. Gladstone had made in Parliament, upon the same subject, in 1870, when he said, in regard to Belgian neutrality:

[5] Hertslet, "Map of Europe," III., p. 1886.

[6] *The Times*, London, August 4, 1914.

"There is, I admit, an obligation of the treaty. It is not necessary, nor would time permit me to enter into the complicated question of the nature of the obligation under that treaty. But I am not able to subscribe to the doctrine of those who have held in this House what plainly amounts to the assertion that the simple fact of the existence of a guarantee is binding on every party to-day irrespectively altogether of the particular position in which it may find itself at the time when the occasion for acting on the guarantee arises. The great authorities upon foreign policy to whom I have been accustomed to listen, such as Lord Aberdeen and Lord Palmerston, never to my knowledge took that rigid, and if I may venture to say so, that impracticable view of the guarantee. The circumstance that there is already an existing guarantee in force is, of necessity, an important fact, and a weighty element in the case to which we are bound to give full and ample consideration."

Sir Edward Grey added to this his own statement, that:

"The treaty is an old treaty—1839. It is one of those treaties which are founded not only on consideration for Belgium which benefits under the treaty, but in the interests of those who guarantee the neutrality of Belgium."

Unfortunately this is true. That treaty is evidently an obligation of convenience. Germany, upon her side, took the same view. The German Chancellor in his speech before the German Parliament alluded in this connection to "the wrong which we were doing in marching through Belgium." The German government declared that "it had in view no act of hostility against Belgium." It expected the Belgians to maintain an attitude of friendly neutrality toward Germany,—in return for which it undertook, at the conclusion of peace, to guarantee the independence of the Belgian kingdom in full. The Chancellor hoped that the Belgian authorities would yield to the inevitable and "retire to Antwerp under protest."

I do not intend to pursue this inquiry in the direction in which it has given rise to the controversy on both sides, and possibly the world over, as to whether the Allies were ready to pass through Belgium if the Germans had not done so. We are concerned merely with the law. Of course, if Belgium had taken the slightest step toward uniting her forces with either of the belligerents as against the others, she would have forfeited her attitude of neutrality and become herself a belligerent, subject to be treated as an enemy. And this would be the end of her independent existence; for that is based upon the neutrality which the convenience of the great powers has determined upon as the condition precedent of her national life.

But, assuming that she committed no breach of neutrality,—what rights has Belgium or Switzerland or any other neutralized territory? It has the right to defend itself, as Belgium has done. She is not obliged to defend herself, but may choose whether she will do so or not. For, if she yield to superior force, that can not be looked upon as an un-neutral act; though it may place her during the war upon the side of one of the belligerents, as is the case of Belgium today in consequence of her defence. Still, Belgium had undoubtedly the right to defend her soil. The law is on her side in that regard.

But, on the other hand, what protection has she? Evidently nothing but the agreement under which she lives,—and that depends either upon the "interests" of the powers who made the agreement, as Sir Edward Grey said, or upon the convenience of respecting it, as the advance of the German army has proved.

In the heat of a savage conflict, the reasons for the agreement are destroyed and the agreement itself is torn to shreds; for there is no one to enforce it. The only force that exists is being exhausted in the war. The neutralized territory has rights that are not only recognized but also defined by international law. It has its guarantees as well,—equally recognized and defined, though, as in the present case, the authority of the law is gone, and how shall a method be found by which to guarantee the guarantees?

PHILADELPHIA,
April, 1915.

THE PRONOUNS AND VERBS OF SUMERIAN.

By J. DYNELEY PRINCE.

(*Read April 23, 1915.*)

The pronouns of a language are relics of its earliest demonstratives. The first desire of the primitive speaker must have been to indicate objects. So soon as nouns had evolved themselves in his mind, the next step was the development of an abbreviated form which could indicate substantives without repeating the noun itself, and these abbreviations or indicators were nothing more than pronouns. It is possible that there existed originally in primitive speech only a single impersonal element of this character, which was at first used, supplemented by gestures, to indicate objects of all three persons. Subsequently, the same syllable may have been tonally differentiated to indicate the 'I, thou, that' idea and still later, additional syllables were called into play to aid in differentiating the first, second and third persons. It is interesting to observe that in the very evidently extremely primitive system of Sumerian pronouns, all the personal particles contain the common demonstrative element *e,* which appears most prominently in the third personal *ene.*

The object of the present paper is to present in a concise form the results of grammatical investigations regarding the Sumerian pronominal particles and also to weigh these theories and conclusions from a philological point of view, especially in connection with the incorporation of the pronominal elements in the verbal structure. It is interesting to note that the distinction between the nearer and farther subject-object, herein noted in connection respectively with the *b* and *n* particles, is a most natural linguistic phenomenon which would have followed almost arbitrarily the evolution of the general demonstrative idea.

The material used in this treatise has been taken partly from the

new vocabularies published in Arno Poebel's "Grammatical Texts,"[1] with the main conclusions of which the present writer is forced to disagree, as the material offered by Delitzsch, Langdon and Prince seems to disprove Poebel's chief thesis of the hidden vowel of the first person.

I.

Separable Pronouns.

Mà-e, 'I,' according to Delitzsch, § 28 = *ma* + demonstrative *-e*. Langdon, p. 102, thinks that *mà-e* was pronounced *mö*, as he regards *a-e* as a diphthong, indicating an *Umlaut*. This is possible, especially as the writing *me-a* = *anaku*, 'I,' also occurs. The pronunciation was more likely *mə̀* than *mö*. The form *mà-e* was invariably used for the *status rectus;* note that in such cases as IV. R. 17, 40–41; *mà-e mu-un-ši-in-gí-en* = *ịaši išpuranni*, 'he has sent me,' the *mà-e* is really a *status rectus* in prolepsis and not an accusative, which would be regularly represented by the oblique *ma* (see just below). It is interesting to notice that Delitzsch gives *me-e* instead of *mà-e* as the usual form, which is again an indication that *mà-e* was not pronounced in two syllables, but really indicated an *Umlaut*. Delitzsch is, therefore, probably right in supposing that the writing *mà-e* really indicated original *ma*, the element of the first person, + the indicative *e*. All authorities are agreed that *a-e* may represent *e* or *ö* (cf. Delitzsch, § 18b).

The oblique form of *mà-e* is generally *ma*, as Poebel: gen. *ma-a-(k)*; cf. *ma-a-kam*, 'it is mine,' Poebel, p. 43; *ma-a-ge-eš ğe-ti* = *aššumia liblut*, 'for my sake may he live.' The Dative is regular: *ma-a-ra, ma-ra, ma-a-ar* (*passim*). In the locative, Poebel finds

[1] The following abbreviations have been used: AJSL: "American Journal of Semitic Languages"; ASKT. = Paul Haupt, "*Akkadische und Sumerische Keilschrifttexte*"; Br. = R. Brünnow, "Classified List of Cuneiform Ideograms," Leyden, 1887; Del. = Delitzsch: Friedrich Delitzsch, "*Sumerische Grammatik*," Leipzig, 1914; EK. = *Eme-ku* dialect; ES. = *Eme-sal* dialect; HT. = ASKT.; JRAS. XVII. = "Journal of the Royal Asiatic Society," quoted Poebel, pp. 63 ff; Langdon = Stephen Henry Langdon, "Sumerian Grammar," Paris, 1911; MSL. = J. D. Prince, "Materials for a Sumerian Lexicon," Leipzig, 1908; P. = Poebel: Arno Poebel, "Grammatical Texts," Philadelphia, 1914; P. AO. 5403: quoted, Poebel, pp. 62–63; P. 142: quoted, Poebel, pp. 57 ff; PSBA. = "Proceedings of the Society of Biblical Archaeology"; Sfg. = Paul Haupt, "*Sumerische Familiengesetze*."

ma-a, 'on me' (not in Delitzsch). The regular accusative is also *ma-a.*

Poebel (p. 42) gives *mu-me-en* as the full separate form of *ma-e,* which clearly contains the first personal element *m(u)* + *me-en* of the verb 'to be' = 'it is I who am' (cf. *s.v. me-ne,* 'we').

The regular suffix of the first person is *-mu,* not to be confused with the third personal *-mu* referred to below. It is now practically established that the first and second persons suffered a change of vowel in the oblique relation, and that the *-mu* in such cases became *-ma;* as *e-ma,* 'in my temple'; *uru-ma,* 'in my city'; *lugal-ma,* 'for my king,' etc. The difficulty in establishing any definite rule in this connection lies in the fact that both *mu* and *ma* appear indiscriminately for both *status rectus* and oblique (see both Langdon and Delitzsch for numerous examples). The probability is that the original usage of the earlier language was *mu* for *rectus* and *ma* for oblique, but, even in the early documents, we find the confusion of forms so evident, as to make it impossible to come to a definite conclusion. The former theory that *-ma* was the ES. form for EK. *mu* is undoubtedly incorrect. On *-ni* = 1 p. suffix, cf. below, *s.v. e-ne,* 'he, she, it.'

Za-e, 'thou,' according to Delitzsch, § 29 = *za* + demonstrative *e,* as in the case of *mà-e,* 'I.' Similarly Langdon, p. 102, thinks that *zà-e* represented *zö,* but this, like *mà-e* = *mǒ,* was probably pronounced *zǒ.* (*ǒ* = *a-e*). *Za-e,* like *mà-e,* was the invariable form of the *status rectus.* In such phrases as *kâtu amâtka* = *za-e e-nim -zu,* 'thy word,' *kâtu* is really the separable pronoun in nominative apposition. Cf. the remarks above on *mà-e* = *ịaši.* Note that the second personal pronoun is also given as *ze,* in *ze-me,* 'thou art' (*passim*) and occasionally *zi-me,* Br., 3387.

The oblique of *za-e* is generally *za;* note Poebel: gen. *za-a(k),* *za-a-a(k);* *za-a-ge;* dat. *za-ra, za-ar; za-a-šu* (KU), 'unto thee' and pure locative *za-a,* 'on thee' (Poebel), a case not in Delitzsch. The oblique *za* is always used with the postposition as *za-da,* 'with thee, from thee,' etc.

Poebel gives also the separable *ze-me-en,* corresponding to *mu-me-en,* 'I' (see, however, *s.v. za-e-me-en, s.v.* the second person plural).

The regular suffix of the second person is *-zu*, with usually oblique *-za*, as in the case of *-me*, *-ma* (see just above). But here also *-zu* is found as both *rectus* and oblique, although *-za* seems to have been the original oblique form. Cf. *mà-e eri-za*, 'I am thy servant' (*-za* for *-zu*, L., § 158); *ga-zu-ta*, 'at thy command' (probably should be *ga-za-ta*, etc.). It is not possible to predicate a regular usage for *-zu; -za*.

E-ne, 'he, she, it'; according to Delitzsch, § 30 = demonstrative *e* + demonstrative *ne* = *nê*, 'this.' This is clearly the same *ne*, seen in the plural of nouns and verbs. Langdon (p. 107) thinks that *e-ne* = a reduplicated *ni* with apocope of the first *n; i. e.*, a sort of plural form. This idea has little foundation, as the demonstrative *e*-element is well established in other forms (as, for ex., *mà-e, za-e, lugal-e*, the king, etc.) Poebel gives no separate form for *e-ne*, the probability being that *e-ne* itself served as such. There is no distinct oblique form of *e-ne* which is declined like a noun: gen. *e-ne-ge* (KIT); dat. *e-ne-ra, e-ne-ir;* loc. *e-ne-a*, 'upon him' (Poebel).

The suffix of the third person has a twofold aspect; viz., 1) *-(a)-ni* and *-ni*, the former being rarer in occurrence than the latter; the oblique of this form is *-na;* and 2) *-(a)-bi* and *-bi*, the former being rarer than the latter; the oblique form of this is *-ba* (Delitzsch, § 37). The same confusion of usage is seen here as that between *-mu, -ma* and *-zu, -za*, fully pointed out by Delitzsch, § 38; *ki-ba*, 'in its place'; *šu-na*, 'into his hand,' regularly oblique, but *a-na* = *abušu*, 'his father' (for *a-ni*) and *dam šà-ga-a-ni*, 'the man of his heart,' instead of *-a-na*, etc. As to the meaning of the *-n-* and *-b-* suffixes, Langdon (p. 105) believed that *-ni, -na* as both noun suffixes and verbal elements, originally denoted animate beings, while *-bi, -ba* indicated inanimates, but the logical continuance of this theory is not borne out by the facts. We may note that in one of Langdon's own examples *bi-e-nad-di-en*, 'he slumbers,' *bi-*, here as verbal prefix, represents an *animate* subject (cf. my review, AJSL. XXVIII. p. 73). Note also HT., p. 76, 1 and 9: *su-mu-ug-ga-ni* and *su-mu-ug-ga-bi*, 'his suffering,' in both cases animate. Delitzsch, § 40, also gives many examples. The suffix *-ni* is used for the first person in Br., 5334: *i-de tum-a-ni* = *ublim pâniya; ud tur-ra-ni-ta* = *ultu ûm cixriku*, 'from the days of my youth'; *lal-a-ni* = *candaku*, 'I am

yoked.' The only possible explanation is that the translator deliberately transferred the persons. The possibility that the *-n-* and *-b-* elements were originally used to denote the remote and nearer subject or object respectively, has already been pointed out by Thureau-Dangin, ZA. XX., pp. 380–404, and fully discussed by Poebel (ZA. XXI., pp. 218–230; Prince, AJSL., pp. 364–365). This theory, although not yet capable of entirely satisfactory demonstration, lends itself more readily to credence than the animate-inanimate idea. In the later language, which represents a period of grammatical decay, the *n* and *b*-suffixes appear to be used arbitrarily. It is probable, however, that in the earlier phases of Sumerian, these endings must have had the force of remote and nearer demonstratives respectively.

Me-ne, me-en-ne, 'we.' Poebel gives *me-en-de, me-de, me-en-de-en,* which, however, should be read *me-en-ne, me-ne, me-en-ne-en.* He uses the *d*-element, because he finds the oblique form *me-en-da-na,* 'without us' (p. 47) and also *nam-da-me-en-da-na,* 'without us'; viz., *nam* negative + prep. *-da* + first person plural *me-en* + prep. *da* repeated + *-na,* probably negative, repeated. Poebel's own form *me-da-nu* (p. 34, line 34), 'without us' clearly shows that the *me-en* in these *me-en-da*-forms is the *me-en* of the first person. Thus, *me-en-da-nu* = *men* first person + prep. *da* + negative *nu.* A form *me-en-de* eliminates the evident combination of *me* = first person + plural *-ne.* Similarly, Poebel's separate forms *me-de-en-de* and *me-de-en-de-en* must be read *me-ne-en-ne* and *me-ne-en-ne-en,* respectively; *me-en-ne* = 'we' + *en,* element of the verb to be; lit. 'it is we who are' (cf. *mu-me-en, s.v. mà-e* above).

According to Delitzsch, *-me-ne,* etc. = *ma* + *ene,* 'I and he,' a sort of exclusive 'we.' But if this were the case, we should expect to find also an inclusive 'we' = 'I and thou,' which would have the form *me-en-zi-en* (or *me-ze*), but this form actually occurs with the equation *attunu* 'you,' plural (just below). It is much more likely to suppose that *me-en-ne, me-en-ne-en* represent a pure plural of the first personal *mê(n)*; *i. e., mê(n)* + *ne* or *ne-ne* + the verbal *-en,* when the form ends in *-n.* The pluralizing of the first person singular occurs for example in Central American Tule *an-mala* = *an* 'I' + the collective *-mala.* Indeed, the form *men-men* is actually

a reduplication of the first personal singular = *me-* + verbal *-n*. We find the reduplicated suffix *-mu-mu* 'our' (see below), which confirms this view.

Me-en-ne declines regularly, although no genitive has been found as yet; probably = *me-en-ne-ge* (KIT); dat. *me-en-ne-ra, me-en-ne-ir;* loc. *me-en-ne-a,* these last two cases being given by Poebel.

The suffix of the first person plural appears as 1) *-mu-mu,* Langdon, p. 109, n. 1, although this is rare; 2) Clay, Miscellaneous Tablets, has found: *dumu-mu-meš* 'our child,' a direct plural of *-mu;* 3) as *-men* : *en-men* 'our lord,' Langdon, p. 103 (Delitzsch, § 33, gives *-men* as frequent in this sense); 4) the common suffix is *-me: ad-da-me* 'our father'; *ama-me* 'our mother'; *ki-me-ta* = *ittini* 'with us,' etc. The curious form *ki-me-ne-ne* = *ittišunu* 'with them,' Delitzsch, § 43, probably was wrongly translated and means 'with us'; *i. e., ki* 'with' + *me-ne-ne,* a pluralized form of the usual *-me.* There seems to be no distinction in these suffixes between *rectus* and oblique. This is clearly indicated by the series of suffixes *an-ne-en, en-ne-en, in-ne-en, me-en-ne-en, un-ne-en,* all which are used for the first person plural (MSL., p. xxii, § 5) and are not honorifics as I thought (AJSL., XXVIII., p. 73). These are merely plural first personal suffixes with possible connecters (cf. just below *s.v. me-en-zi-en*). The *-nen* element which appears in all of them must represent *-me-n.*

Me-en-zi-en = *attûnu* 'you' (given by all sources) and also *zi-ne* 'you,' a real plural of the second personal element *zi* (*ze* = *za-e*), Langdon, p. 104. Note the parallel *me-ne* 'we.' In view of the fact than *za-e-me-en* also = *attûnu,* IV. R. 21, 1 B. rev. 3, clearly = *za-e* + *men* = 'thou and I,' it is probable that *me-en-zi-en* also = 'I and thou' (*me,* 'I' + verbal *-en* + *zi*(*ze*), 'thou + verbal *-en*). But this *za-e-me-en* is equivalent to Poebel's full form of *za-e* (see above *s.v. za-e*). It is impossible that *za-e-me-en* could have been a second personal singular separate form and at the same time a second person plural! If it were really used in both senses there must have been a different tone for each usage of *men* = respectively the verb 'to be' and the first person. Note that the odd reading NI-*e-me-en,* HT. 139, § 7, clearly = *za*(*l*)*-e-me-en.*

Of *me-en-zi-en* no genitive has been discovered, but it probably

was *me-ne-zi-en-ge* (KIT); dat. *me-en-zi-en-ra* and *za-ra-an-zi-en* (!); loc. *za-a-an-zi-en*. In these two latter forms, we have a reduplication of the second person; *i. e.*, *za* + dat. *-ra* + verbal (*a*)*n* + the second personal *zi* with verbal *en* = *zaranzen* and *za-a* loc. + (*a*)*n* = second person + *zi* with verbal *en* = *zânzen*.

The suffix of the second plural is *-zu-ne*, as *mu-lu-zu-ne*, 'your lord,' Langdon, p. 104. Note that in Delitzsch, "*Sumerisch-Akkadisch-Hettitische Vokabularfragmente*," p. 19, the form *á-zu-šú-ne-a-áš* = *ana ittikunu*, 'for your wage' = the suffix *-zu-ne*, with infixed preposition *šú* (KU) + directive *a-áš*, an unusual and interesting example of infixation. The suffix *zu-ne-ne* often occurs, Delitzsch, § 42: *u-gu-zu-ne-ne* = *elikunu*, 'upon you'; *nam-en-un-un-zu-ne-ne* = *macartikunu*, etc. Here we have plainly the pure plural of the second personal element and no indication of 'thou and I.'

As in the case of the first person plural, there seems to be no distinction between *rectus* and oblique. This is indicated by the series of suffixes similar to those just cited in connection with the first person plural; *-ab-ci-en, -an-ci-en, -en-ci-en, -ib-ci-en, -íb-ci-en, in-ci-en, -me-ci-en, -me-en-ci-en* and *-un-ci-en*. The forms *-me-ci-en* and *-me-en-ci-en* may contain an 'I and thou' element. These all represent the second personal suffix with possible connecters.

E-ne-ne-ne, 'they'; according to Delitzsch, § 32; *ene* + *enene*, 'he and they,' but this form is more likely to be *ene*, 'he, she, it' + the reduplicated plural element *-ne*, as in the case of *me-ne* and *-zu-ne-ne*, cited above. The short form *e-ne-ne* is also common. Poebel gives *e-ne-ne-ne* as the full separate form, but without sufficient foundation, as either *e-ne-ne* or *e-ne-ne-ne* might have served in this capacity, as in the case of the singular *e-ne*.

The third person declines regularly; gen. *e-ne-ne-ge* (KIT); dat. *e-ne-ne-ra, e-ne-ne-ir;* loc. *e-ne-ne-a*.

The third plural suffix, as in the case of the third person singular, is twofold; 1) (*a*)*-ne-ne*, the *a* not being always present, in fact it is usually part of the prolonged root, as *dug-ga-nene*. It appears regularly *šu-ne-ne*, 'their hand'; *gir-ne-ne*, 'their foot; *ki-ne-ne-ta* = *ittišunu*, 'with them' (on *ki-me-ne-ne* = *ittišunu*, Delitzsch, § 43, see above *s.v. me-en-ne*). 2) The endings with the *b*-element: *bi-e-ne-ne, bi-e-ne*, Delitzsch, § 43; *be-ne-ne*, Langdon, p. 108, and

be-ne, Langdon, p. 108, are also common: *sib-bi-ne,* 'their shepherd'; *muğ-bi-ne-ne* = *elišunu* (Langdon, p. 108). The distinction between remote and nearer subject and object, respectively *-ne-ne* and *-bi-ne,* is no more logically carried out in the later language than in the case of *-ni, -bi* of the third person singular (*q.v.*), but their original remote and nearer force seems just as probable.

The third person plural possessive is frequently expressed by the singular suffixes of the third person: *-ni, -bi,* a phenomenon which

TABLE OF PRONOUNS.

	I	*Thou*	*He, She, It*
Nom.	*ma-e* Separate: *mu-me-en*	*za-e* Separate: *za-e-me-en*	*e-ne*
Gen.	*ma-a-(k)* *ma-a-ge*	*za-a(k)* *za-a-a(k)* *za-a-ge*	*e-ne-ge*
Dat.	*ma-a-ar* *ma-ra* *ma-a-ra*	*za-ra* *za-ar*	*e-ne-ra*
Loc.	*ma-a*	*za-a* (*za-a-šù*, 'to thee')	*e-ne-a*

	SUFFIXES	SUFFIXES	SUFFIXES
Rectus[2]	*-mu*	*-zu*[2]	(*a*)*-ni, -ni*[4] (*a*)*-na, -na*
Oblique	*-ma*	*-za*	(*a*)*-bi, -bi*[4] (*a*)*-ba, -ba*
	(*-ni* very rare and probably an error)		*-mu*[5]

	We	*You*	*They*
Nom.	*me-ne, me-en-ne, me-en-ne-en*	*me-en-zi-en*	*e-ne-ne* *e-ne-ne-ne*
Gen.	[x]*me-en-ne-ge*	[x]*me-en-zi-en-ge*	*e-ne-ne-ge*
Dat.	*me-en-ne-ra* *me-en-ne-ir*	*me-en-zi-en-ra* *za-ra-an-zi-en*	*e-ne-ne-ra* *e-ne-ne-ir*
Loc.	*me-en-ne-a*	*za-a-an-zi-en*	*e-ne-ne-a*

SUFFIXES[3]	SUFFIXES[3]	SUFFIXES[3]
-me-en	*-zu-ne*	*-(a)-ne-ne, -ne-ne*
-me	*-zu-ne-ne*	
-mu-mu, *-mu-meš* } rare		*-be-e-ne-ne*[3] *-be-ne-ne,* etc.

CONNECTING SUFFIXES	CONNECTING SUFFIXES
an-ne-en, en-ne-en, *in-ne-en, me-en-ne-en,* *un-ne-en*	*ab-ci-en, an-ci-en,* *en-ci-en, ib-ci-en,* *ib-ci-en, in-ci-en,* *me-ci-en, me-en-ci-en,* *un-ci-en*

[2] Confused usage.

[3] No distinction between *rectus* and oblique.

[4] Probable distinction between nearer and farther subject and object.

[5] Used only with participles, so far as is known. See below.

is seen in other languages, as, for example, in Central American Tule, where *a'ti, i'ti* can indicate both 'he, she, it' and also 'they' (Prince, Amer. Anthropologist, XV., p. 484; the *a'ti, i'ti* -element may be pluralized by the collective suffix *-mala,* which, however, is often omitted).

II.

Sumerian Verb with Pronouns, with References to Following Commentary.

I-Thee

Ga-class: *ga-mu-ra-ab-dím = lu-bu-ša-ku-um,* 'I will (let me) make for thee,' P., No. 142, rev. 2, 10.

Mu-class: *nu-mu-ra-te-mà-dè(ne)-en = u-la e-ṭe-xi-a-kum,* 'I shall not go to thee,' P. AO. 5403, 6. *xu-mu-ra-ab-gă(r),* 'I gave thee as a present,' P., p. 103.

Mi-class: *mi-ni-max-en,* 'I made thee great therein (for it),' P., p. 112.

I-Him

Ga-class: *ga-an-na-dím = lu-bu-su-um,* 'I will make it for him,' P., No. 142, rev. 2, 15.

Mu-class: *xu-mu-na-du,* 'I built for him,' P., p. 102. *mu-na-du,* 'I built for him,' P., p. 102. *xu-mu-ni-max = lu-ša-ti-ir,* 'I will increase for him' (or) 'therein,' P., p. 102.

Mi-class: *mi-ni-í = a-na-ku šu-a-ti šu-a-ti,* 'I it for him,' P. JRAS., XVII., 65, 4, 23. *mi-ni-du = al-bi-in,* 'I moulded it for him' (or) 'therein,' P., p. 102. *xu-mi-ni-in-tax = lu-um-mi-su,* 'I supported it,' P., p. 102.

Ne-class: *ne-gí-a,* 'I restored it (Clay).'

Ba-class: *ba-a = a-na-ku šu-a-ti(+ -ti;* probably *= šuati šuati),* P. JRAS., XVII., p. 65, 4, 19. *ba-ni-í = a-na-ku šua-ti šu-a-ti,* 'I it for him,' P. JRAS., XVII., p. 65, 4, 28 (also *ba-ni-e* ditto). *ba-an-na-te-en e-te-xi-šum,* 'I went to him,' P. AO., 5403, 8.

Bi-class: *bí = a-na-ku šu-a-ti,* 'I it (or) him,' P. JRAS., XVII., p. 65, 4, 13. *bi-i = a-na-ku šu-a-ti,* 'I it (or) him,' P. JRAS., XVII., p. 65, 4, 14.

I(n)-class: *i-ni-í = a-na-ku šu-a-ti šu-a-ti,* 'I it for him,' P. JRAS., XVII., p. 65, 4, 22. *in-na-ni-í = a-na-ku šu-a-ti šua-ti u*

a-na-ku šu-a-šum, 'I it for him and I to him it,' P. JRAS., XVII., p. 65, 4, 30.

I-HIM

sag-túm-ma i-ni-in-ga = ma-gi-ir-tam ak-bi-šum, 'I spoke favorably to him,' P. No. 142, rev. 3, 21. *in-na-te-en = e-iṭ-xi-šum,* 'I have gone to him,' P. AO. 5403, 2.

IM-CLASS: *ù-gul im-ma-an-mà-mà,* 'I asked him,' P. p. 102.

THOU-ME

MA-CLASS: *nam-ma-te-má-dé (ne)-en = la ta-ṭe-xi-a-am,* 'do not come to me,' P. AO. 5403, 5.

MU-CLASS: *nam-mu-un-xa-xa-en = la tu-te-bi-da-(an-ni),* 'mayst thou not be lost to me,' P. No. 142, rev. 3, 8.

THOU-HIM

POSTPOSITIVE CLASS: *dím-(ma)-na-ab = e-bu-su-um,* 'make for him,' P. No. 142, rev. 2, 14. *gur-an-ši-ib,* 'turn to him,' P. No. 142, rev. 2, 16. *te-a-na = ṭe-xi-šum,* 'go to him,' P. AO. 5403, 1. *na-an-na-te-mà-dè(ne)-en = (la)te-ṭe-(xi)-šu-um,* 'do not go to him,' P. AO. 5403, 4.

MI-CLASS: *mi-ni-e = at-ta šu-a-ti- šu-a-ti,* 'thou it it,' P. JRAS. XVII. p. 65, 4, 25.

BA-CLASS: *ba-e = at-ta-ku (sic !) šu-a-ti,* 'thou it,' P. JRAS. XVII. p. 65, 4, 20.

BI-CLASS: *bi-NE = at-ta šu-a-ti,* 'thou it,' P. JRAS. XVII. p. 65, 15. *bi-e = at-ta šu-a-ti,* 'thou it,' P. JRAS. XVII. p. 65, 16.

I(N)-CLASS: *i-ni-e = at-ta šu-a-ti šu-a-ti,* 'thou it it' or 'it for him,' P. JRAS. XVII. p. 65, 4, 24. *in-na-ni-e = at-ta šua-ti šu-a-ti,* 'thou it for him,' P. JRAS. XVII. p. 65, 4, 32. *in-na-te-e-en = te-iṭ-xi-šum,* 'thou hast gone to him,' P. AO. 5403, 9, also 'go to him,' ditto, 2. *in-da-má-e-en = ta-ša-(ka)-aš-(šu)-mu,* 'thou shalt place it upon him,' P. AO. 5403, 10.

HE-ME

MA-CLASS: *ma-an-si = i-din-nam,* 'he gave it to me,' P. p. 110. *igi . . . ma-ni-in-du-a,* 'when he looked upon me,' P. p. 104. *igi . . . mu-ši-in-bar-ra,* 'when he looked upon me,' P. p. 102. *ma-a-ar ma-an-du-ga,* 'when he commanded me,' P. p. 104. *ma-ra ma-an-du-ga,* 'when (to build) she ordered me,' P. p. 105. *xa-ma-ab-*

dím-e = *li-bu-ša-am*, 'may he make for me,' P. No. 142, rev. 2, 22.

MU-CLASS: *mu-ub-dim-e* = *i-bu*(*pi*)-*ša-am*, 'he made for me'; P. p. 57, rev. 3, 19, renders 'makes'(?). *nu-mu-ub-dím-e* = *u-la i-bu-ša-am*, 'he did not make for me,' P. No. 142, rev. 3, 20. *sag--túm-ma mu-un-ga* = *ma-gi-ir-tam ik-bi-a-am*, 'he spoke favorably to me,' P. No. 142, rev. 3, 21. *sag-ki . . . mu-ši-in-bar*, 'he looked on me,' P. p. 105. *mu-na-an-si*, 'he has given to me,' P. p. 110. *nam mu-un-tar*, 'he determined fate for me,' P. p. 105.

MI-CLASS: *nam-mu mi-ni-in-tar-ra*, 'after he had determined fate for me,' P. p. 105.

HE-HIM

MU-CLASS: *mu-na-ni-in-gi-gi*, 'he replied to me,' P. p. 93. *u . . . mu-na-an-si-ma-ta*, 'after he had given to him,' P. p. 105. *mu-na-ni-in-du*, 'he had built for him therein,' P. p. 105. *mu-na-an-ši-in-gar*, 'he made it for him,' P. p. 106.

MI-CLASS: *mi-ni-in* = *šu-u šu-a-ti šu-a-ti*, 'it for him,' P. JRAS. XVII. 65, 4, 27. *mi-ni-in-tar-ra*, 'when he had determined it for him (it),' P. p. 112.

BA-CLASS: *ba-on* = *šu-u šu-a-ti*, 'he it,' P. JRAS. XVII. p. 65, 4, 21. *ba-an-na-te* = *i-ṭe-xi-šum*, 'he went to him,' P. AO. 5403, 7.

BI-CLASS: *bi-in* = *šu-u šu-a-ti*, 'he it,' P. JRAS. XVII. p. 65, 4, 17–18. *šu-ni bi-in-si-a*, 'after he had placed it in his hand,' P. p. 105.

IB-CLASS: *ib-ri-tuk*, 'he shall receive for it,' (Clay).

I(N)-CLASS: *i-ni-in* = *šu-u šu-a-ti*, 'he it it,' P. JRAS. XVII. p. 65, 4, 26. *in-na-ab-si-mu* = *in-na-din-šu*, 'he shall give it to him,' P. p. 94. *in-na-ab-gi-gi* = *ip-pa-alšu*, 'he shall answer it to him,' P. p. 94. *in-na-ab-gur-ri* = *u-ta-ar-šu*, 'he shall return it to him,' P. p. 94.

THEY-ME

MU-CLASS: *xu-mu-ši-in-bar-ri-eš* = *lu-ip-pa-al-su-um*, 'they have looked upon me,' P. p. 104. *sag-e-eš xu-mu*-PA-TUG-DU-*eš*, 'they have given it to me as a gift,' P. p. 104.

BI-CLASS: *šu-mu-šù bi-in-si-eš-a*, 'when they gave it into my hands,' P. p. 104.

THEY-THEE

PRECATIVE CLASS: KA *xa-ra-ab-ša-ša-gi-ne* = *li-iš-te-mi-ga-kum*, 'may show reverence unto thee,' P. p. 110.

THEY-HIM

PRECATIVE CLASS: *xe-e-en-na-ab-dím-e-ne* = *li-bu-šu-šum*, 'may they make for him,' P. No. 142, rev. 2, 17.

MU-CLASS: *mu-un-ni-in*-PA-TUG-DU-*a*, 'when they had given to her as a gift,' P. p. 112. *mu-na-an-si-mu-uš-a* = *i-ti-nu-šum*, 'when they had given it or gave it to him,' P. p. 104. *mu-na-an-gi-ni-eš-a* = *u-ki-in-nu-šum*, 'when they had made secure for him,' P. p. 104.

I(N)-CLASS: *in-na-ab-ka-la-gi-ne* = *u-dan-ni-nu-šum*, 'they shall pay him,' P. p. 104. *na-an-na-ab-dím-e-ne* = *la i-pi-šu-šum*, 'may they not make for him,' P. No. 142, rev. 2, 18.

COMMENTARY.

1. *Ba-*, 'I' (*s.v.* I-HIM); cf. also IV. R. 14, obv. 20 a: *ki-bi-gar-ra ba-ni-ib-dur-ru* = *ina tâkulti lûšešib*, 'I will invite (them) to a feast'; probably first person, but the text is broken.

2. *Ba-*, 'thou' (*s.v.* THOU-HIM); not an uncommon usage. Cf. AJSL. XIX., § 20; IV. R. 17, 45 a; in IV. R. 30 nr. 3, rev. 15, there occurs a series of *ba——ne* forms all = 2 p. It is possible that the Assyrian scribe changed them from a 3 p. which perhaps was used for a general "you" like German *man;* French *on.*

3. *Ba-*, 'third person' (*s.v.* HE-HIM); occurs *passim.*

4. *Bi-*, 'I' (*s.v.* I-HIM); an unusual prefix in this sense.

5. *Bi-*, 'thou' (*s.v.* THOU-HIM); an unusual prefix in this sense. *Bi-*, as a *prefix*, is unusual in any câse, even as the third person, as it is a common third personal *suffix.*

6. *Bi-*, . . . *-eš*, 'they' (*s.v.* THEY-ME); not common, although, if *bi-* is used in the singular 3 p., it is natural to find *bi-*, . . . *eš* for the plural.

7. *Ga-*, 'I' (*s.v.* I-THEE; I-HIM); a very common first personal prefix, probably from the cohortative *ga*; in fact, *ga-* was really cohortative originally, although it is frequently used in the sense "I will." Cf. Del., § 157, and AJSL. XIX., §23. It also = 1 p. in HT. 119, obv. 22: *ga-nu ga-ni-lax-en* = *alkam i nillikšu*, 'come let us go.' Note that is 1 p. plural here. On the other hand, *ga-* is used for the 2 p., IV. R. 11, 45 b: *en-nun ga-ne-tuš* (KU) = *ana maccarti tûšêšib*, 'thou shalt sit in the watch'; cf. AL.[3] 134, obv. 1: *an-sud ud-ag bil-gim sar-ki-ta za-e ši-in-ga-me-en bil* = *nûr šamê ša kîma išatim ina mâtim napxat attîma*, 'the light of the heaven which like fire in the land shines art thou, fire.' *Ga-* also may be used for the 3 p.; IV. R. 11, 19–20 b: *mu-uš-ku-pi azag-ga-na-ta a-an ga-mu-ri-a-bi* = *ina uznišu elliti minam ixsusa*, 'what has he planned with his brilliant ears.'

8. *Im-*, 'I' (*s.v.* I-HIM); cf. also IV. R. 6, 41 b: *ki-ta im-mi-in-ri*, 'I

placed it at the bottom,' but used with a preceding *mà-e*, 'I.' *Im-*, however, can mean "thou"; II. R. 16, 16 e: *er* (A-SI) *im-ma-an-šeš-šeš* = *tabakki*, 'thou weepest.' *Im-* is very common as a third personal prefix.

9. *In-*, 'I' (*s.v.* I-HIM). Very rare. Poebel's examples are the best instances of this use.

10. *In-*, 'thou' (*s.v.* THOU-HIM); IV. R. 7, 30 a: *nin ma-e nin-zu-a-mu za-e in-ma-e-zu* = *ša anâku idû atta tîdi*, 'what I know thou shalt know' (='for me'=*-ma-e-*?); cf. AJSL. XIX., § 28. *In-* is commonly used with the third person.

11. *In-*, . . . *-ne*, 'they' (*sv.* THEY-HIM); a logical third person plural.

12. *Ma-*, 'me, to me' (*s.v.* HE-ME); for this usage Poebel's examples are best. Note also *ma-an-se* = *iddinšu*, 'he gave it to him' with the third person, Br. 4418.

13. *Mi-*, 'I' (*s.v.* I-THEE). Poebel's example is the only one known to me.

14. *Mi-*, 'me' (*s.v.* HE-ME; HE-HIM). Note that the *-nin-* here = '*me.*' *Mi* is most common with the third personal sense, Br., p. 546.

15. *Mi-*, 'thou' (*s.v.* THOU-HIM); cf. also IV. R. 24, nr. 3, 6–7: *tul-tul-aš mi-ni-in-šid* = *tilâniš tamnu*, 'thou regardest it as a ruin,' but points back to a 2 p. *-zu* in line 3.

16. *Mu-*, 'I' (*s.v.* I-THEE; I-HIM); very common use (see AJSL. XIX., § 32).

17. *Mu-*, 'thou' (*s.v.* THOU-ME); only in Poebel, so far as I have met it.

18. *Mu-*, 'he' (*s.v.* HE-ME; HE-HIM); *passim;* AJSL. XIX., pp. 217–218.

19. *Mu-* . . . *-eš*, 'they' (*s.v.* THEY-ME); a natural plural of *mu-*, 'he.' Note *mu-* . . . *-uš*, the same plural, as *mu-* . . . *eš* with vowel harmony; *uš* for *eš*.

III.

Analysis of Material.

The prefixes, infixes and suffixes shown by the above table may be grouped alphabetically as follows:

an-ši-ib, '(turn thou) it to him' = *šib*.

ba- = 1, 2 and 3 p. subject.

ba-an-, 'he it.'

ba-an-na-, 'he it; he for him.'

bi- = 1, 2 and 3 p. subject.

bi- . . . *-eš* = 3 pl. subject.

ga- = 1 p. subject.

ga-mu- = 1 p. subject: 'I will.'

ĝen- . . . *-e-ne* = 3 p. pl. precative subject.

i- = 1 p. subject; cf. *i-ni-in*.

i(b)- = 3 p. subject.

im- = 1, 2 and 3 p. subject.

i-ni-in-, 'I to him.'
in- = 1, 2 and 3 p. subject.
in-na-, 'I to (for) him.'
in-na-ab-, 'he (they) to him.'
ma- = 2 p. subject; 3 p. subject: 'he to me.'
ma-ab-, 'he it for me.'
ma-an-, 'she (he) . . . me' (acc.).
ma-ni-in-, 'he upon me.'
mi- = 1, 2 and 3 p. subject.
mi-ni, 'I thee therein; I it for him; he it for me.'
mu- = 1, 2 and 3 p. subject. Note also the following:
mu- . . . *-eš* = 3 p. subject.
mu-na-an-, 'he to (for) him; he it to me; they it for (to) him.'
mu-na-ni-in-, 'he it for me.'
mu-ni-, 'I for him.'
mu-ši-in-, 'he for me.'
mu-un-, 'he for (to) me.'
mu-un-ni-in-, 'they it for (to) her (him).'
-na-, 'for (to) him.'
-na-ab-, 'it for (to) him.'
-ni-i-, 'for him'; *mi-ni-i-*, 'I for him; thou for him'; *ba-ni-e-*, 'I it for him'; *i-ni-i*, 'I it for him.'
-ra-, 'to thee.'
-ra-ab-, 'it for thee.'

Analyzing the above elements stlil further, we observe that the first personal subject may be denoted by: *ba-; ga-; ga-mu-; im; in-; i-; mi-; mu-*.

The second personal subject may be denoted by: *ba-; bi-; in-; ma-; mi-; mu-; mu--eš* (pl.).

The third personal subject may be denoted by: *ba-; bi-eš* (3 pl.); *i(b)-; in-; ma-; mi-; mu-*.

In other words *ba-, im-, in-, mi-*, and *mu-*, may indicate the 1, 2 and 3 persons indiscriminately, and that *ma-* = 2 and 3 persons, while *ga-* is almost always used for the 1 p.

Nor is the problem made easier by the tabulation of the 1, 2 and 3 personal objective infixes; viz., 1 p. object: *ma-*, 'he to me'; *ma-ab-*, 'he it for me'; *ma-an-*, 'she (he) . . . me' (acc.); *ma-ni-in-*,

'he upon me'; *mi-ni-*, 'he it for me'; *mu-na-an-*, 'he it for me'; *mu-ši-in-*, 'he upon me'; *mu-ub-*, 'he for me'; *mu-un-*, 'he for (to) me.'

The second personal object shows: *mi-ni-*, 'I thee therein,' but consistently *-ra-*, 'to thee; thee'; *-ra-ab-*, 'it for thee.'

The third personal object is seen in *ba-an-*, 'he it'; *ba-an-na*, 'he it; he it for him'; *in-na*, 'I to him'; *mu-na-an-*, 'he to (for) him; they it for him'; *mu-un-ni-in-*, 'they it for her (him).' The element *-na* clearly = 'to him,' as *na-ab* = 'it for him'; *-ni-i-*, 'for him,' as *ba-ni-i*, 'I it for him'; *i-ni-i*, 'I it for him'; *mi-ni-i-*, 'I for him; thou for him.'

We find in these forms the duplicate *mi-ni-i-*, 'I thee therein' and 'I for him' = 1 and 3 object; *mu-un-*, 'he for me,' but *mu-na-an-*, 'he it for me' and 'he it for him.'

Poebel's table of pronominal elements as used by the verb (p. 45) is most ingenious, but not satisfactory, as will be shown. His classification is as follows:

	Infixed.	Enclitic.	Absolute.	Suffix.
1 p.	'	*m*	*m* (*de* and *en*)	*en*
2 p.	*e*	*z*	*z* (and *en*)	*en*
3 p.	*n*	*n*	*e*	*e*
Collective	*b*	*b*		

This he has elaborated from his Paradigms (pp. 70 ff); thus: *ni-la-en*, 'I pay'; *n* is preformative + the *i* which contains the 1 p. '; *la* = *root* + *en*-suffix of the 1 and 2 p.; *ni-la-en* also = 'thou payest'; only here, he thinks, that his 2 personal *e* is contained in the *i* of *n-i*. *Ni-la-e* also means 'he pays,' where the *n* = preformative of the third person + connecting vowel *i* + root *la* + 3 personal suffix *-e*. The analysis of the forms, just given is my own, made from what I believe to be his theory. The '-vowel for the first person again appears in the simple forms *i-dim*, 'I made'; the *e*-vowel of the second person in *e-dim*, 'thou madest' and the *n* of the 3 p. in *indim*, 'he made' (p. 78). Similarly *a-tum*, 'I brought' (*a* = *a*'); *a-mên*, 'I am' (Clay); *e-tum*, 'thou didst bring' and *an-tum*, 'he brought' (p. 80) seem to indicate the correctness of his idea. But, without entering more deeply into this ingenious and carefully thought out theory, it may be demolished by the simple fact that *a-* (= *a*'), *e-* and *n-* do not always mean the 1, 2 and 3

persons, although *a-* and *e-* *usually* occur in this sense. Thus, *a-* indicates the 3 p.: *a-rim-rim-ne* = *iṭ-ṭi-bu-(u)*, 'they have been immersed'; *kas-?* *a-ab-du* (KAK) = *ši-ka-ra* *i-ba-ba-di* and *a-ne-in-gi-* = *ik-?-šu*, both clearly third persons, although the meaning of the stems is unknown (cf. Br., p. 548). On the other hand, *a-* generally indicates the 1 p. (Prince, AJSL. XIX., p. 211). The prefix *e-* is frequently used of the 3 p. as: *e-ag*, 'he made'; *e-gaz*, 'he killed'; *e-gen*, 'he went' (Delitzsch, § 135 and § 184a). As for *n(i)*, Poebel himself gives examples cited above of *n(i)* used for both the 1 and 2 persons, while for the 3 personal use, cf. Br., p. 543: *ni-zu* = both *i-du-u*, 'they know' and *ti-di*, 'thou knowest'; *ni-gal* (IK) = *i-ba-aš-ši*, 'it is,' etc. *ad nauseam*. In other words, *a*, *e* and *n(i)* appear used for all three persons indiscriminately with a preference in favor of the 1, 2 and 3 persons respectively. On the other hand, is Poebel correct in supposing that the suffixed forms *-en* attached to verbs are characteristic of the 1 and 2 persons only? As in the case of *a*, *e* and *n* they appear indiscriminately for all three persons: *ni-la-en*, 'I pay'; 'thou payest,' as cited above, but *mu-un-tag-en* = *in-naq-qu-u*, IV. R. 19, 48 b; *mu-un-ši-in-gí-en* = *iš-pu-ra-an-ni*, 'they have sent me,' Br., p. 560. As to Poebel's 3 p. *-e*, it, of course, occurs often with the 3 p.; cf. *til* (BE)*-e* = *ig-da-mar*, Br., 1499, etc., but also *an-na-ab-uš-e* = *tu-ša-ax-xa-za-šu*, 'thou shalt cause him to seize it'; it is also a frequent sign of the imperative, as *kú-e* = *akul*, 'eat thou'; *uš-e* = *ri-da-an-ni*, 'have connection with me,' Br., 553.

There can be no doubt that Poebel is right in giving *m-z* and *n-b* as the respective characteristics of the endings of the postpositive conjugation, as *-mu-*, *-zu-* and *-ni*, *-na-* and *-bi-*, *-ba-* are the ordinary 1, 2 and 3 personal suffixes, respectively, of the postpositive *ḥâl*-clause; yet even here we find a variation, as the third person also appears with the ordinarily first personal *-mu* in relative clauses. This is the so-called *-mu* of the third person which I believe I was the first to call attention to (MSL. XXIX, § 32). The best illustration of it will be seen in the following phrases from IV. R. 27, No. 1, 4–11:

šinig-ga šar-šar a nu-nag-a-mu
bi-i-nu ša ina mu-sa-ri-e me-e la iš-tu-u

the grain which hath drunk no water in its bed;
suğur edin-na pa nu-sìg-ga-mu
kim-mat-su ina ci-e-ri ar-ta la ib-nu-u
whose bud in the field no shoot has borne;
GIŠ-A-AM *šita*(RAT)*-na ba-nu-su*(*g*)*-ga-mu*
il-daq-qu ša ina ra-ṭi-šu la i-ri-šu
the sprout which in its water-ditch is not planted;
GIŠ-A-AM *ur-ra ba-ab-bu-ra-mu*
(*il-daq-qu*) *ša iš-da-nu-uš in-na-as-xu*
the sprout whose roots have been torn away;
gu šar-šar-ra a nu-nag-a-mu
qu-u ša ina mu-sa-ri-e me-e la iš-tu-u
the vegetation which in its bed has drunk no water;

A similar construction to the above is undoubtedly that in ASKT., p. 122, 16: *eri-zu-ka ág-gig-gá ak-a-mu* = *ana ar-di-ki ša ma-ru-uš-ṭum ep-šu*, 'unto thy servant (fem.) who has (lit. 'makes' = *ak*) sickness.'

It is perfectly evident from the above examples that we have here a purely relative *-mu* used with participles. This is probably identical in derivation with the demonstrative *mu-* in the regular relative pronoun *mu-lu* and also with the common *mu*-prefix of verbs. It is quite possible that this relative *-mu* was used to indicate all three persons, like the *mu*-prefix in verbs.

What then are we to conclude as to the pronominal use with the Sumerian verb? Is it possible to imagine a verbal system with no fixed method of expressing the pronouns? The existence of the practically fixed second personal value of the infix *-ra-* and of the very common use of *ga-* as a first person would lead us to suppose that the verbal prefixes were really not indeterminate pronominally, even though Delitzsch lays down the rule that there is no second personal conjugation in Sumerian (p. 102).

The existence of third personal elements has long been recognized. The difficulty lies in the apparently indiscriminate use of many verbal prefixes for all three persons and the fixation of this usage by Poebel's undoubtedly valuable equations. The question now is whether Poebel is right in supposing that there underlies in every case of a first personal usage the '-vowel, *i. e.*, that *mu-*, 'I'

stood for *mu-*', while *mu*, 'he' did not contain this element. This is equally true of the *e*-prefix of the second person varying with *i*, cited by Poebel as characteristic. Are we to understand an *e*-element hidden in every second personal equation; *ba-, im-, in-, mi-, ma-, mu-*? The latter question must be answered in the negative, because, as just shown, *e* was not used exclusively of the second person. An examination of the paradigms as given by me in this paper will show the improbability of such a proposition.

The first thought which strikes the philologist studying this maze of apparently contradictory forms suggests the theory that in Sumerian, as in other languages, person in the verb must have expressed by the tone. This idea I suggested in AJSL. XIX., pp. 205–206, but no Sumerologist has ever gone into the matter. All scholars in this line have preferred, either to deny the distinction of pronouns by the verbal prefixes or else to suggest a difference in quantity (Paul Haupt, Sfg., p. 19, n. 6; Bertin, PSBA. V (1882–3), pp. 19 ff). But a difference in quantity or "accent," as some call it, would have been indicated at least by a prolongation of the vowel of the prefix. Real voice-tone would not have been so designated, any more than it is Chinese Wen-Li to-day. Grammatical tones actually exist in African Yóruba, as *ile ṛe*, 'thy house' but *ile ṛê* (another tone), 'his house'; in this language *o* = 'thou' but *ô* (another tone) = 'he, she, it.' Nothing could be more suggestive than this parallel (cf. S. Crowther, "Grammar of the Yóruba Language" (London, 1852), p. 12). I cite it, not of course with the intention of connecting Sumerian with Yóruba, but simply to demonstrate the possibility of toned grammatical elements which do not occur in Chinese. The three persons expressed by *ba-, im-, in-, mi-* and *mu-*, the two persons by *ma-* and the similar apparently indiscriminate use of the infixes, noted above, all point only to such a solution, which is far more reasonable than the idea that hidden vowels exist in such prefixes and infixes. If these vowels were present, how were they distinguished? There is nothing in the inscriptions to betray their existence. The Chinese do not indicate tones in their writing, because they are as readily understood by the reader of a living language, as an English reader understands the distinction between words of identical sound and difference of mean-

ing such as 'so, sow' and 'sew'; 'low'='below' and the verb 'low,' etc. Similarly, the Babylonian priest to whom Sumerian was, if not in later times actually a living tongue, at least a pronounced idiom, would never have thought of indicating the tonal differentiation of the grammatical verbal elements. The very poverty of Sumerian phonetically and the apparent monotony of its consonantal elements go to show the necessity of supposing some special unindicated means of differentiation. There seems to be every reason to suppose that such elements cited above as *ba-, im-, in-, mi-, mu-* indicating the first, second and third persons in the verbal scheme must have been tonally differentiated.

There are only about eleven distinguishing consonantal elements in the language; viz., *b,* probably = near object and near demonstrative; *d* = partitive; locative; means; *g* = precative and intentional; hence, future (= also *ng* = *n*); *ğ* = pure precative optative, indicated herein by *x*; *l* (rare) = *n; m* = demonstrative and relating; *n,* probably = remote object and demonstrative; *r* = ethical dative; motion, direction towards, and perhaps rhotacism for *z* in the second person *-ra; š* = direction towards, similar to *r,* with which it may be connected etymologically; *t* = 'in' or 'out of'; location in general; *z* = pure second person, the only fixed consonontal grammatical value. Combine with these elements the vowels *a* = direction and *i* (*e*) = completed action, past and future, having a force like the Slavonic "perfective" forms, not forgetting that *i* may be the harmonic equivalent of *e* and *u* of *a,* and we get a reasonable explanation of most of the prefixes and suffixes of the language, particularly of the verbal prefixes treated above. See for a full discussion of these points, Prince AJSL. XXIV. pp. 354–365, and also in Encyclopaedia Britannica, XXVI, p. 77.

Poebel's infixes (pp. 70 ff), all which are, of course, well known, I will amplify by the following examples for the sake of clearness: *na,* 'to him'; *in-na-an-ba-e* = *uqassu,* 'he gives to him' = *na,* 'to him' + *n,* 'it' remote; *in-ne-la-e,* 'he will pay to them' (*-ne*); cf. *mu-ne-gen,* 'he went to him'; note that *šin* = 'them,' HT., p. 46, 25; *in-ši-in-se,* 'he gave to them'; *ma-la-e,* 'he will pay me'; here the *m* stands for the 1 p. + the directive *a; mu-ra-la-e,* 'he will pay thee'; the *mu* contains the demonstrative *m* + the tonal vowel of the

third person + the 2 p. *-ra-; in-ši-la-e,* 'he will pay to him': *i,* the perfective vowel + *n* = remote object 'it' + *ši* directive; *mu-ši-la-e,* 'he will pay to me'; the tonal *mu* of the 1 p. + directive *ši; mu-e-ši-la-e,* 'he will pay for thee': demonstrative *m* + tonal *u* of the second person + perfective *e; i(n)-ni-la-e,* 'he will pay upon (= for) it' = perfective *i* + remote object *n* + perfective *i* again + *ni,* really = 'therein'; *i(n)-na-ni-la-e,* 'he will pay to him upon it'; same as the above with the directive *na*-insert; *ib-ta-la-e,* 'he will pay from it (out of it)'; perfective *i* + nearer object *b; i(n)-na (b)-ta-la-e,* 'he will pay to him from (out of) it'; same as above with directive *na* + nearer object *b; ib-da-la-e,* 'he will pay together with it'; better 'by means of it'; *ib* as above with the *da* of means; *mu-e-da-la-e,* 'he will pay together with thee' demonstrative *m* with tonal *u* of the second person + perfective *e* + *da* as above. Poebel's whole set of infix-paradigms may be explained satisfactorily following this system.

THE HALL AND CORBINO EFFECTS.

By E. P. ADAMS.

(*Read April 22, 1915.*)

About four years ago Professor Corbino[1] described some effects which are closely related to the Hall effect. These new effects all have to do with the production of a secondary circular current in a metallic disk when a primary radial current is sent through it, and the disk placed in a magnetic field perpendicular to its plane. The only metal in which Corbino was able to detect any of these effects was bismuth, perhaps owing to lack of sensitiveness in his methods, but more probably because he seems to have neglected to take the precaution of preventing circular currents in a parallel disk used to lead the radial current into the disk under investigation. These circular currents would produce an effect which would largely balance the effect sought.

Last year Mr. Chapman and I[2] measured this Corbino effect in twelve different metals. In two other metals, tin and zinc, the effect was too small to measure. The method of measurement consisted in measuring the current induced in a coil of wire placed parallel to the disk when the radial current was reversed about twenty times a second by a rotating commutator.

The result of these measurements showed that the circular current C, produced, was proportional to the magnetic field, H, and to the radial current, I, or

$$C = aHI.$$

In the magnetic metals and in bismuth a is not constant but it depends on the magnetic force. In all the other metals tried a appears to be constant.

In order to compare this effect with the Hall effect, we may

[1] *Physikalische Zeitschrift*, XII., pp. 561, 842, 1911.
[2] *Philosophical Magazine*, XXVIII., p. 692, 1914.

assume that the effect of the magnetic field is to produce an electric intensity at right angles to both the magnetic force and to the primary electric intensity, and proportional to their product and the sine of the angle between them. This we may take to be:

$$E' = cVHE,$$

where V stands for the vector product. Applying this to the Corbino effect in a circular disk where r_2 is the external radius and r_1 the internal radius we find:

$$E = \frac{I}{2\pi ktr},$$

where I is the whole radial current, k, the specific conductivity, and t the thickness. Then the transverse electric intensity is

$$E' = \frac{cIH}{2\pi ktr}$$

and the whole circular current:

$$C = \frac{c}{2\pi} \log \frac{r_2}{r_1} \cdot IH.$$

Therefore the constant a is equal to $(c/2\pi) \log (r_2/r_1)$

$$c = \frac{C}{IH} \frac{2\pi}{\log \frac{r_2}{r_1}}.$$

We may now make the same hypothesis about the Hall effect. Here it is known that if a current I flows through a rectangular sheet metal of length l, breadth b, and thickness t, there is a transverse difference of potential given by

$$e = R\frac{HI}{t},$$

R being the Hall constant. The transverse electric intensity is now

$$E' = \frac{cHI}{kbt},$$

and thus the transverse difference of potential is

$$e = \frac{c}{k}\frac{HI}{t}.$$

Thus

$$R = \frac{c}{k}.$$

The constant c may be determined from both the Hall and Corbino effects. Experiments that Mr. Chapman has recently been making show that c is the same when measured by the two effects.

Now it is known that the Hall effect varies in sign from metal to metal. This change in sign may be introduced in the hypothesis by supposing that the constant c varies in sign for different metals. The experiments that have been made show that the Corbino effect changes in sign with the Hall effect. Thus there can be little doubt that these two effects are essentially the same, and that any explanation of one effect will explain the other.

Corbino also showed that when a disk carrying a radial current was placed in a magnetic field so that the normal to the disk made an acute angle with the direction of the field, a torque was brought into play tending to make the disk parallel to the field. If ϕ is the angle between the normal to the disk and the magnetic force, the mutual energy of the circular current and the magnetic field is

$$W = \frac{c}{8\pi} \cdot IH^2S \cos^2 \phi,$$

where S is the area of the disk. Thus the torque tending to increase ϕ is

$$-\frac{\partial W}{\partial \phi} = \frac{c}{8\pi} IH^2S \sin 2\phi.$$

Mr. Smith has succeeded in measuring this torque in four or five different metals, including bismuth, and the values of c calculated from his results are in good agreement with those obtained from the measurement of the circular current.

The production of a circular current in a disk by a magnetic field acting on a radial current implies an increase in its resistance.

This increase may be readily calculated. The rate of heat production by the radial current is

$$\frac{I^2 \log \frac{r_2}{r_1}}{2\pi kt}.$$

By the circular current it is

$$\frac{2\pi C^2}{kt \log \frac{r_2}{r_1}} = \frac{c^2 I^2 H^2 \log \frac{r_2}{r_1}}{2\pi kt};$$

the total rate of heat production is thus

$$\frac{I^2 \log \frac{r_2}{r_1}}{2\pi kt}(1 + c^2 H^2).$$

If k' is the conductivity of the disk in the magnetic field we may write the total rate of heat production when a radial current I is sent through it

$$\frac{I^2 \log \frac{r_2}{r_1}}{2\pi k' t}.$$

Thus

$$\frac{k}{k'} = \frac{\sigma'}{\sigma} = 1 + c^2 H^2 \quad \text{or} \quad \frac{\partial \sigma}{\sigma} = c^2 H^2.$$

In this expression σ is the specific resistance of the metal and σ' its effective specific resistance in the transverse magnetic field. Now according to this view the resistance of a conductor should always be increased by a magnetic field. It is known, however, that with some metals notably iron and nickel, the resistance is decreased in a transverse magnetic field. Furthermore, the increase of resistance calculated from this formula is very much less than the increase actually observed. In the thought that the change of resistance might be dependent on the geometrical form of the metal Mr. Lester has measured the effect of a transverse magnetic field on the resistance of a number of metals, using disks with a radial current.

His results are in good agreement with previous measurements made with wires and strips. For example, in the case of bismuth, using the same disk that was employed to measure the circular current he found, for a field $H = 5{,}000$, $\delta\sigma/\sigma = 0.16$. Now $c = 210^{-5}$ for $H = 5{,}000$, so that $c^2H^4 = .01$. It is thus certain that some other influence is effective in causing the main part of the change of resistance of a metal in a magnetic field; it is very probable that the field affects the molecular structure of the metal.

The interpretation of these results from the point of view of the electron theory of metallic conduction is unsatisfactory. I have worked out their theory[3] assuming free electrons in the metal that collide with the metallic atoms and obtained very simple expressions for the number of electrons in unit volume and their time between collisions. The numbers so obtained are of the same order of magnitude as have been obtained by other methods. But the difficulty of accounting for the difference in sign of the effect for different metals on any such simple theory indicates that if we are to hold to the electron theory of metallic conduction other forces than those resulting from collisions like those between hard elastic spheres must be supposed to act upon the electrons. The surprising thing is that so much can be explained by the simple theory of electrons when all such forces are neglected.

We have seen that the Corbino effect is, essentially, the same as the Hall effect. In its measurement and interpretation the Corbino effect has some important advantages over the Hall effect. In the first place it is not necessary to use the very thin films that are required to produce measurable Hall effects. And in the second place the absence of the free transverse boundaries render the interpretation of the Corbino effect simpler than that of the Hall effect.

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[3] *Philosophical Magazine*, XXVII., p. 244, 1914.

SOME RESULTS FROM THE OBSERVATION OF ECLIPSING VARIABLES.

By RAYMOND S. DUGAN, Ph.D.

(*Read April 24, 1915.*)

At Princeton we have been using for over ten years a stellar photometer devised by Pickering and similar to the one used for so many years to such good purpose by Wendell. This photometer has many virtues and but few vices. Its construction is such that the observer has the very comfortable conviction that nearly all the sources of systematic error he can think of are being rendered innocuous by the program of observation. In accuracy it is apparently excelled by the electrical photometers alone.

A large part of our researches at Princeton has for some time been the observational and theoretical study of eclipsing variables. From the beginning we were of the opinion that the patient distribution of observations repeatedly throughout the entire period of light-variation might very possibly bring us several facts to repay us for the great labor involved.

The first star subjected to this process was *RT* Persei.[1] 14,464 measures made during the years 1905–8, combined into a mean curve, showed at once the existence of a secondary minimum, 0.13 of a magnitude in depth, and with satisfactory distinctness a slight change in brightness between eclipses. This latter change was interpreted to mean that the two stars are ellipsoidal and brighter on the sides facing each other. An asymmetry, established with considerable certainty in the curve of primary eclipse, was found to prevail throughout the entire curve. Combining this with the knowledge of one component of the eccentricity derived from the observed displacement of secondary minimum toward the preceding

[1] See Contributions from the Princeton Univ. Obs., No. 1.

primary minimum, Shapley has recently explained this asymmetry as a Periastron effect—an increased brightness of the stars when nearest each other. There is another possible explanation which I shall consider in connection with the third star.

In 1909 the series of 18,384 measures of z Draconis was completed.[2] Evidence of a very shallow secondary minimum, only six or seven hundredths of a magnitude in depth, displaced slightly toward the following primary minimum, was clearly found. Ellipticity of figure and exchange of radiation were again demonstrated. These two effects have been abundantly verified in other cases.

The theoretical representation of the observations of both *RT* Persei and z Draconis just at the beginning and end of eclipse is a little unsatisfactory—the theoretical curve starts to drop more rapidly than the observed curve. This is possibly evidence that the stars are darkened toward the limb—like the sun.

It was hoped that the observations of *RV* Ophiuchi would not only demonstrate the existence of darkening toward the limb but would determine approximately the degree of darkening. But darkening seems to be very elusive sometimes. The observed secondary minimum is barely deep enough for the uniform solution, and a darkened solution requires a still greater depth. The well-determined primary minimum is strongly asymmetrical. The inter-radiation effect is about the same as in the other two stars, while the ellipticity is much greater. This latter fact when considered with the anticipation that the ellipticity effect would in this case be too small to detect—on account of the large distance between the component stars—is somewhat disconcerting. Then too there are several well-defined hollows in the curve—a conspicuous one just after primary and another just after secondary—and a long stretch before secondary when the star is much brighter than before primary. The only satisfying method of solving the curve seemed, therefore, to be a least square solution of the whole curve—a procedure hitherto avoided. The final theoretical curve, representing our present knowledge of the causes of light variation of these stars, is the result of the solution of forty-two equations with seven

[2] See Contributions from the Princeton Univ. Obs., No. 2.

unknowns. The probable errors are of satisfactory smallness, considering the amount of asymmetry.

It seems necessary in this case to face the fact of asymmetry unflinchingly. The secondary minimum gives no evidence of an eccentric orbit and the consequent possibility of a magnified periastron effect. The most conspicuous asymmetry is a greater brightness from middle of primary eclipse through to secondary than on the other side. A sine term of the first order with an amplitude of three hundredths of a magnitude takes care of the greater part of this. The existence of this sine term would probably indicate that the advancing side of the bright star is brighter than the following. If such were the case, then the loss of light during the early stages of eclipse—when the brightest part of the disk is being covered—and the gain of light right after totality would both be more rapid than would be the case if the disk were uniformly bright, as was assumed for the least square solution. The divergence from the theoretical curve of the well observed branches of primary minimum is in this anticipated direction and it is of about the right amount. A similar asymmetry has already been remarked in the curve of *RT* Persei and is probably also, at least in part, due to this cause. This suggested explanation is of course not new, but the evidence is apparently strong that the advancing side of the brighter component of some Algol variables is brighter than the following side. After this sine term is removed from the curve of *RV* Ophiuchi, there seem to be other changes in brightness of an amount small but seemingly guaranteed by the probable error.

In the system *RT* Persei, one star is one and one third times as large and five times as bright as the other; in z Draconis, one star is thirteen times as bright as the other but just about equal to it in size. The eclipse is very deep and nearly total. In *RV* Ophiuchi one star is twelve times as bright as the other but smaller. The brighter star is entirely hidden behind the fainter for about an hour. The two stars are farther apart than in the other two systems but they are apparently much more elongated.

During the seven years since completing the light-curve of *RT* Persei I have observed through an occasional primary minimum both visually and photographically. Recently I have observed two

secondary minima. The eclipses are now coming over forty minutes earlier than they are predicted by the elements determined from the original series of observations. This is very surprising in view of the accuracy with which the elements were determined. These elements were determined mainly from a large number of well observed minima grouped quite closely about two epochs about 700 periods apart. If the time of eclipse is fixed by the observations within a half-miniute in each of these two regions, then the period is known to one-tenth of a second. In cases where both branches of the minimum were continuously observed it seems hardly possible to change the observed time of any individual minimum by more than one or two minutes, and still represent the observations closely.

The photographic record of *RT* Persei, generously furnished me by Professor Pickering, showed that at —7,500 *P*, or 17½ years before the zero epoch, the eclipses were about 100 minutes late. The average period, then, is nearly a whole second shorter than the period determined from my original series of observations. This shorter period should have caused my observed minima to run off ten minutes from the predictions during the interval of 700 periods. This is intolerable. Beside my own observations, there are a good many observations by Wendell and several by Graff available. Of course a single estimated photographic magnitude is not nearly as accurate for determining the time of minimum as a series of photometric observations right through the eclipse.

Making now the correction to the shorter, average period, I have plotted the new residuals, and find that two periodic terms, one running its course in 12,000 eclipse-periods, or 27½ years, the other in one-third this time, or 4,000 periods, with coefficients of twelve and five minutes respectively, going through their zero values on the up grade together, fit the observations pretty well. The smaller period is of the order of magnitude of that to be expected from the revolution of the line of apsides caused by the observed prolateness of homogeneous stars. The amplitude is of the size to be expected from the smallest value of the eccentricity in accord with the observed shift of secondary minimum.

The important question, and one which is difficult to answer, is whether the time of the secondary eclipse shifts in either of these

periods. The revolution of the line of apsides in the shorter period would cause the time of secondary minimum to oscillate back and forth to an extreme of ten minutes before and after the midway point between successive primaries.

The evidence is both scarce and somewhat uncertain. The entire secondary eclipse amounts to a drop of but little over a tenth of a magnitude. An isolated photographic estimate of brightness is of little value here. Even from a continuous series of photometric observations, it is difficult to fix on the time of mid-eclipse within several minutes. My own observations, weighted according to the apparent certainty with which they determine the time of mid-eclipse, indicate rather strongly the shift in the time of secondary eclipse in the shorter of the two periods. The few observations by Wendell do not furnish any very strong evidence for or against this result. In no case were his observations carried through both branches of the eclipse, and consequently they do not determine the time of the eclipse with much accuracy. What disagreement there is, is in the same direction as in the observations of primary eclipse—for some reason the Wendell times of eclipse are nearly all earlier than mine. The evidence in hand at present points to a revolution of the line of apsides of *RT* Persei. I hope to observe occasional primary and secondary minima of this star during the next few years.

As a bi-product, I have determined the photographic curve from the Harvard observations and compared it with the visual curve of primary minimum. According to the observations, the eclipses in the two regions of the spectrum are of the same duration and the same depth, but the curves follow different paths. This is too strange a result to be taken very seriously, considering the paucity of the photographic material. I have, however, one minimum which I observed through a blue color screen made for the purpose. Through this filter the minimum was observed about 0.15 magnitude deeper than without the filter. So I conclude that the observations at the bottom of the photographic curve, which are few in number, are to be disregarded and the curve extended to the greater depth given by the color-screen observations. The difference in the character of the two curves indicates that when one star is in great part covered up, the light of the system is more reddish than when

they both shine undimmed. Either one star is redder than the other, or the eclipsed star is redder toward the limb than at the center.

z Draconis has been an equally interesting surprise. The average period is quite a little longer than was supposed. Some time ago it shortened up with great rapidity. The top and bottom of this sharp decline are well determined by observations from quite a variety of sources. The two sine terms combined in this case have periods of 7,200 and 2,880 eclipse-periods and nearly the same coefficients as in *RT* Persei—ten and four minutes respectively. The prolateness and eccentricity of z Draconis are about the same as those determined for *RT* Persei.

The secondary minimum of z Draconis is only 0.06 magnitude—half as deep as that of *RT* Persei; the observations of the secondary minimum are few and were all taken within a brief interval, and they furnish very little evidence. This star must also be kept under frequent observation. When observing this star with the 23-inch it requires a determined effort to see it at all when in the middle of its deep eclipse.

The greater depth of the photographic eclipse comes out very nicely in the case of z Draconis. As the eclipse increases, the light from the star becomes redder and redder. At deepest phase more than half the light is known to come from the fainter star. It is apparently much redder than its far brighter companion, a fact which is doubtless to be expected.

Lastly, in reducing the observations of *RV* Ophiuchi I found it necessary to predict the minima with a sine term of about 1,600 periods, and of small amplitude. The photometric history of *RV* Ophiuchi is short and incomplete compared with that of *RT* Persei and z Draconis, and no conclusions can safely be drawn from this result.

PRINCETON UNIVERSITY,
April, 1915.

SOME PRESENT NEEDS IN SYSTEMATIC BOTANY.

By L. H. BAILEY.

(*Read April 23, 1915.*)

If an editor were to survey the families and genera and species of the vegetable kingdom, he would find himself making comparisons and drifting to conclusions respecting the character of the systematic work and the worth of various contributions. Many of these conclusions he might not be able to analyze. They might be very much in the nature of impressions, and yet they might be felt so strongly as to be convictions. It is a vast field that his oversight would cover, and the bases of comparisons would be of the most various kinds, yet the convictions in very many cases would be concrete. It may be well to consider for the moment some of these possible convictions, of course in no spirit of captiousness, but to bring other points of view on some of our common problems, even though these points of view may not always be capable of direct application.

Very likely, his first feeling would be a consciousness of the great variety in the methods of the monographs. The systematic work is rapidly specializing, and the specialists make their own criteria. The result is a marked diversity in the work, which all the efforts at standardization do not very much control. Probably, Bentham and Hooker's "Genera Plantarum" is the last of the comprehensive works to be brought to a completion by a single person or by two or three persons working as one. This is succeeded by the editorial work of Engler and Prantl in "Die Natürlichen Pflanzenfamilien," and later in more detail by Engler in "Das Pflanzenreich." Floras of countries and regions tend more and more to be constructed editorially, with contributions by specialists. All this results perhaps in closer work in the specialties and the details, but it may lack in coordination and in the balancing of the parts.

Probably all the larger conclusions by our hypothetical editor would be derived from this general situation. No longer do we have the controlling authority of one man, holding the work steady and maintaining a homogeneous method. I well remember a remark that Asa Gray made about his Compositæ, on which he had worked so long and so lovingly, seeing the end of his time and foreseeing the change of his method. I remember also that in those days I was somewhat violently interested in nomenclature and I proposed to publish on it; but Gray gently dissuaded me: it was some years before I understood why.

A Situation in Nomenclature.

In proportion as we lose the influence of a single controlling personality, or of a few personalities working in an understood harmony, do we resort to arbitrary and conventional methods of codification. This is well illustrated in the convulsions in nomenclature in recent years. In this country, for example, with the passing of Gray, we began to give up the combination of two words as the name of a plant, and to substitute the oldest specific name brought down through any number of genera. Intrinsically, one method is as good as the other, but we sought to arrive at uniformity by rigidly adopting one of them. A train of difficulties has followed this and other innovations, and instead of finding ourselves in full harmony of action, with one uniform practice in nomenclature, we have two or three or several practices, and to a considerable extent each worker making his own. The present situation in nomenclature is a vivid illustration of the failure of arbitrary means of standardization. The situation also has a social significance, as I shall attempt to suggest.

The probability is that we should have arrived at our destination sooner and with no greater confusion if we had allowed the situation to work itself out without formal regulation, recognizing more fully the principle of usage which in the end controls all language. We have probably made a mistake in endeavoring to substitute arbitrary priority for stability; at all events, we might have saved ourselves the very amusing exercise of upsetting a well established name for the purpose of substituting an older name in order that

we might make the name stable. It looks now as if usage were after all to control in the end, and in some regards quite independently of arbitrary regulations. The principle of undeviating priority has not yet controlled for any length of time in the development of language. It is a false premise.

I am not now arguing for a return to any older or prior method, nor in challenge of any current practice, and certainly not in criticism of any group of workers, for we shall probably outgrow our conventionalities sooner by working with them rather than against them. But I must protest, as I have protested many times before, against the assumption that the names of plants belong to botanists to do with them as they will. This is only another way of saying that these latinized names of plants are rightfully a part of language and are not mere formulæ or symbols to be used only by insiders. We desire that the public shall use this language. We publish our manuals with this purpose. We try to make plant books simple, that they may be popular. We take pains to spread the knowledge of plants and thereby to promote the love of nature. There are thousands of persons who sell plants, and the names become established in trade and represent commercial values. These values cannot be shifted readily from name to name; and if one makes a plea for correct nomenclature in plantsmen's catalogues and lists, one receives the reply that it is scarcely worth the while seeing that the names change so frequently. The custom of shifting the names is undoubtedly directly responsible for much of the disregard of new nomenclature on the part of dealers; and we must remember that the use of these kinds of names among the people is probably promoted more by the plant dealers than by the botanists. I judge that the botanists have not yet succeeded in securing the active and free cooperation of this great class of people.

Of course we are to recognize that much of the change is inevitable, that, in fact, it is a consequence of new and closer studies of the groups, resulting in a clearer understanding of generic and specific limitations. This is a contribution to knowledge which everyone must accept. But there is a class of changes which does not have this justification. I am conscious, in making inquiries,

that the first thought of some particularists appears to be a desire to see whether it is possible to change the names.

Nor am I yet ready to leave this subject. From a successful and sincere public lecturer, who is trying to lead the people to a knowledge of animals and plants, I had a request for aid containing the statement that he could devote only a little time daily "to the study of Latin and I want to get only a sufficient knowledge of it to enable me to know why the gipsy moth is called (*Porthetria dispar* L.) and whether *Raphamus raphanistrum* means a plant, an insect or a tribe of elephants." This person, of course, had not had a college training in these particular subjects, but he is not ignorant or inattentive. He writes that he has about 2,000 bulletins, many bound volumes and a special cyclopedia, nearly all of which material is classified, using a card-index. "It has taken a lot of work to do this but as I can spare from farm labor only about an hour each day for study I find the index is a great time saver by showing me just where to find what I want." This man will accomplish much with his methods of contact. But consider the position of this man if to a complicated system of nomenclature we add a continuous tendency to change; and I think it is fairly our obligation to consider his position.

When we feel within us the desire to change the names of genera and groups, let us think well of this man and his carefully considered hour,—what it would mean to him in cross-referencing, in indexing, in the readjusting of his work. If it is to bring new knowledge that we cannot so well record otherwise or indispensable definitions, very good; but the burden of proof always rests on the new name. The work with names is fascinating, even captivating, and every change identifies the worker with it; but we are not to forget that some of this work is likely to be of the kind that, in other fields, might be called pedantry.

Bear with me further while I call your attention to the fact that we are not only changing our plant names with apparent disregard of the users of them, but that we are also making them more complicated. To the name of the plant,—genus and species,—we add the authority. We now omit the punctuation and thereby make the

author in effect a part of the name. When the combination of two words was held to constitute the name of a plant, the author of the combination was sufficient for identification; but with the single-word system we carry the author of both the original specific name and of the new combination, and the whole becomes something like a complicated formula. This is a convenience to the worker with plant names, but he is not the only party concerned; his needs may be served in the citation of the synonomy. His obligation to the public is to present the simplest possible name and the least involved. If the history is to be retained in the name-compound, where may we not stop and how complicated may our formulæ finally become? We may in time evolve a phraseology, or an algebraic form, as complicated as some of the pre-Linnæan customs. We are really confusing two things,—nomenclature and bibliography. We should separate citation from nomenclature. We have no right to inflict the public with our taxonomic book-keeping.

There are three pressing needs in our present systematic botany, as I see it. One of these needs I have now tried to suggest, which is the urgency to subordinate the nomenclature question. This is specially important in a democracy, where we desire to give all qualified persons equal chance, where we are supposed to remove hindrances and arbitrary domination by central authorities and to allow the people to express themselves freely. The public has real rights in the names of plants. Soon we must stop playing with names.

A Situation as to Species and Genera.

The oversight that we assumed in the beginning would undoubtedly discover other interesting situations in our systematic work. What these comparisons might be would depend, of course, on the particular person who made them; but in respect to the American work, with which at the moment we are mostly concerned, any person could not fail to admire the quality of the monographs and lesser contributions. Although systematic botany may occupy a subordinate place in our teaching, it is receiving extensive and very expert attention both from amateurs and from those attached officially to the great collections, and the published work is such as to

give us much pride. Ability of a high order continues to express itself in this field.

We have noted the tendency to specialize. Persons become expert in certain detached groups of plants. We become most skillful in detecting the differences that may distinguish species, but it may be doubted whether we are equally skillful in bringing together the agreements that may formulate genera. We seem now to be discovering separateness. It does not follow that one who has nice judgment on species necessarily has equal authority on genera. The tendency to break up our old groups into many genera, is apparently the result of the application of the species-habit. It is a great question whether the method of separation is the proper one to apply equally in these two kinds of cases.

Perhaps we cannot hope for much result in the standardizing of the species-conception by our methods of herbarium work, but it ought not to be difficult to arrive at some kind of an agreement on genera. We may well consider the advisability of being progressive in searching out the ultimate specific units—so far as there are such units—at the same time that we hold a conservative attitude on genera, for we can scarcely assume that there are ultimate generic lines. Thereby we might make a truthful presentation of the vegetable kingdom at the same time that we avoid vast changes in nomenclature.

A Situation as to the Living Material.

With the needful specialization of the systematic work, we find ourselves with very unequal treatment in the different groups. This inequality is perhaps the most outstanding characteristic of our present phytographical publication. It is impossible at present to compile a general work with any clear approach to uniformity of handling in the different genera and families. This is due in part to the fact that some of the groups have been recently worked over whereas others still retain a traditional treatment. Nor is it desirable that there shall be rigid codification on genera, for we need the judgment of different workers and this necessarily leads to non-uniformity; the specialist is entitled to his method; and yet the inequalities in interpretation appear to be so great in many cases as to amount to inharmony and even to confusion.

While there is more hope in the standardizing of genera than of species, it is within the possibilities to arrive at some kind of agreement on specific values, but this is not to be expected as a result of codification or regulation: it must be a real agreement by men who are brought together on a new kind of study of a common line of problems.

As I have already indicated, I would not expect or even desire a dead uniformity of treatment in any range of systematic work, and least of all in species. It would be a great misfortune to lose the expression of personality in even such formal work as this. But there is need of a closer understanding as to the essential facts in the treatment of the members of a genus. If one were to look over *Erythrina,* for example, one would find about 50 species recognized, native in warm countries in the two hemispheres; and while there is much uncertainty as to the characters of given species, one would not find very wide disagreement between the different authors. If next one were to look on *Eschscholtzia,* one would find a wholly different state of things, notwithstanding the fact that this genus is confined to western North America. Gray saw about a dozen species in this genus; Greene, with more material to work on, saw 112 species; and Fedde sees 123. Jepson, who has studied them with care in the field, is not able to see a great number of species, although he finds numberless seasonal and other forms; and he does not see much hope in solving the *Eschscholtzia* puzzle by the usual study of herbarium material but rather by "combined field and cultural studies."

And here is the particular suggestion I desired to make in the writing of this paper,—that a few groups be worked out very carefully by growing the plants under observation and as far as possible under conditions of control and always, of course, in comparison with living feral material. Such studies might require some years, even in a relatively small group: very good—the results would be all the more convincing. If a half dozen groups could be worked over in this way, with discussion of the living material by standing committees of some recognized association, we should very likely arrive at a basis of judgment such as the present collecting and inci-

dental field notation and indoor study of dried material can never give us. The conclusions,—or the points of view, if conclusions were impossible,—would be invaluable in bringing us to an understanding and therefore to a substantial agreement on some of the matters that are now most perplexing us. This is now the greatest need in systematic botany.

This means that we should now study life histories with the purpose to apply the knowledge in systematic work. We shall come to the end in due time of the inventory process in describing new species. After a time we shall consider it to be scarcely worth the while to carry the separative process very much farther, and we shall then undertake a synthetic process of building up the forms into species-values. The current studies of variation and of plant-breeding are bringing us to a new point of view: it is now time that we begin the incorporation of these methods into our systematic work.

ITHACA, N. Y.,
April 23, 1915.

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THE VARIABLE STARS *TV, TW, TX* CASSIOPEIÆ AND *T* LEONIS MINORIS.

By R. J. McDIARMID.

(*Read April 24, 1915.*)

The four Algol variable stars *TV, TW, TX* Cassiopeiæ and *T* Leonis Minoris have been under observation by the writer for the last three years with the polarizing photometer attached to the 23-inch equatorial of the Princeton Observatory.

The total number of measures made on the four systems is over 35,000, distributed among the four stars as follows, *TV* 9,920, *TW* 13,728, *TX* 8,486 and *T* Leonis 3,792. The light curves of the first three are well defined, while the observations on the last system are not so complete. The periods of the light variation with the exception of *TV* Cass. have been determined from my visual observations combined with photographic measures kindly sent me by Professor Pickering, of Harvard.

The systems will be discussed in slightly different order than the above, the more important left to the last.

The system *TW* Cass. has been observed by other astronomers, and notes published pronounced it as irregular in its variation, later Zinner found it regular in its variation and of the Algol type with a period $1^d\ 10^h\ 16.6^m$. From the discussion of the Princeton observations I found that the period was double the published period and instead of one eclipse there were two differing by 0.05 magnitude. The double period is confirmed by the three observed phenomena, 1st, the difference in depth of the two minima of 0.05 mg.; 2d, the interval from primary eclipse to secondary is 7.8 minutes longer than from secondary to the following primary; 3d, the primary eclipse is 36 minutes longer in duration than the secondary. It is from the knowledge of the last two facts that we are able to determine both components of the eccentricity—the quantities e and (longitude of periastron). The period is 2 days, 20 hours, and

33.6 minutes and the two eclipses have a depth of 0.62 mg. and 0.57 mg. respectively. From a discussion of the light curve following the theory as outlined by Professor Russell in *A. J.*, 36, 5; 36, 1 results were obtained giving the dimensions of the system in terms of the radius of the orbit. It was found that the two stars were of nearly the same size and had the same surface brightness.

In the case of *T* Leonis Minoris as in *TW* Cass. we have two minima, they are however of very different depth, the primary having a loss of 2.46 magnitudes while the secondary has only 0.05 magnitudes. The period is 3 days, 0 hours, 28 min., and 38.0 sec., and is accurately known. Combined with the visual observations I have used the Harvard photographic measures as far back as 1889, and have been able to establish a definitive period. The observations are not so complete as in the other systems, the length of the period being so nearly three days; also weather conditions at special times have entered largely into this.

From a study of the light curve along the lines of the eclipsing theory it has been found that the stars are of nearly the same size but are very different in surface brightness, the ratio being 1 : 18.

The third system *TV* Cass., whose period, $1^d\ 19^h\ 30^m\ 11.7^s$, has long been known, having been observed by Ashbury and Yendell, was placed under observation in October, 1913, at Dr. Shapley's suggestion. At that time nothing was known about the secondary eclipse. From my observations it was found that a secondary eclipse of 0.09 magnitude did really exist, coming 21 minutes before the time of mid period. The orbit of the system like that of *TW* Cass. is eccentric, but in this case the components of the eccentricity can not be separated.

In the two previous stars it was found from the light curve that the stars were of constant brightness between eclipses. The light curve of *TV* Cass. is somewhat different as there seems to be a gradual rise in the curve between primary and secondary eclipse which corresponds to an increase in brightness of the system. The explanation is, that the radiation of the bright star on the side of the fainter one as they approach the time of secondary eclipse tends to brighten its surface and thus give rise to the phenomenon observed in the light curve. From a study of the light curve it was found

that the fainter star was twice as bright on one side as the other. The stars are nearly equal in size with a ratio of surface brightness of 1:5.5. In this system the depth of the primary eclipse is 1.05 magnitudes and its duration a little over 6 hours.

The last system, *TX* Cass., is the most interesting of the four stars treated here. It was announced some years ago as being an irregular variable; later its period was given by Zinner with the note, that the period was probably changing. It was partly on account of these published notes that a thorough study of the light curve of the star was carried out. Owing to the nature of the variation the star has proved to be a difficult system for photometric study. The eclipses, primary and secondary, which undergo a loss of light of 0.54 and 0.33 magnitudes respectively and last over 18 hours, are difficult to observe, in fact it is impossible to obtain a complete minimum on any one night even during the long nights of winter. The Harvard photogarphic measures have again proved to be of extreme value and by combining them with the visual observations I was able to establish a definitive peroid. The period is 2 days, 22 hours, 14 min., and 41.7 sec.

In the systems so far discussed the stellar disc was considered of uniform surface brightness. Assuming this to be the case with the system *TX* Cass. it was found that the observations could not be represented at all satisfactorily; the deviations were in many cases three times the probable error. On the other hand assuming the stellar discs to be similar to the sun bright at the center and decreasing in brightness toward the edge, a very satisfactory representation of the observations was found. The hypothesis of darkened discs seems to be the correct one as it is confirmed by the nature of the eclipses; the secondary eclipse is total with a constant phase of six hours while the primary eclipse has no constant phase and the curve is distinctly round bottomed showing that the variation is continuous. This condition would exist with darkened discs in case of an annular eclipse, and since the secondary is total our natural and legitimate conclusion is that the primary is annular. The system *TX* Cass. seems to offer very strong evidence in support of darkening toward the limb in stellar systems.

The light between eclipses does not remain constant; the light curve is distinctly bowed up, showing the stars are elliptical in shape and have their greatest brightness when we see them broad side on. This is the condition midway between eclipses. From the study of the light curve it was found that the stars are very different in size, with the stars nearly of the same brightness having a ratio of 1 : 1.5. The ellipticity of the stars can best be shown by giving their dimensions expressed in terms of the radius of the orbit.

	Major Axis.	Minor Axis.
Big star	$a_1 = 0.567$	$b_1 = 0.519$
Small star	$a_2 = 0.295$	$b_2 = 0.270$

The stars in this system are very close together.

AN INTERPRETATION OF STERILITY IN CERTAIN PLANTS.[1]

By E. M. EAST.

(*Read April 23, 1915.*)

It is obvious that it is impossible to investigate the cause of sterility in hybrids by the pedigree culture method when such sterility is complete. Occasionally, however, one finds hybrids which are not wholly sterile. Such is the case in the historic cross, *Nicotiana rustica* L. × *Nicotiana paniculata* L. This hybrid holds an enviable position in experimental botany, since it was the first artificial hybrid to be studied. It was made by Kölreuter in 1760 and was studied by him for several years by means of back crosses with each parent.

This cross I repeated in 1909, using as the *N. rustica* parent a small variety *N. rustica humilis* Comes obtained from Dr. Comes through the kindness of Dr. D. G. Fairchild. It has now been studied through five generations both in the field (general morphology) and in the laboratory (histology and cytology). The essential points noted, as I see them, are as follows:

Two species giving extremely uniform progeny when selfed have, when crossed, given an intermediate F_1 population as uniform as themselves, and an inordinately variable F_2 population.

The germination of F_2 seeds varies in different samples from 20 to 60 per cent.

Practically no two F_2 plants are alike, and the parental forms are recovered once in every 100 to 200 F_2 plants.

In F_1, from 1 to 6 per cent. of the ♀ gametes are functional. It is impossible to determine the percentage of viable ♂ gametes formed from the pollen mother cells, but from 2 to 6 per cent. of the

[1] It is impossible to reproduce the photographs shown by means of lantern slides, but an illustrated paper giving the details of the investigation is to be published shortly.

pollen found is morphologically perfect. The maturation difficulty in spermatogenesis is largely at the first spermatocyte division.

F_1 plants are as fertile *inter se* as in back crosses with either parent.

Segregation of determiners for fertility occurs in F_1, so that by recombination some perfectly fertile plants are obtained in F_2.

Nearly all fertile F_2 plants selfed give only fertile progeny. Occasionally a fertile F_2 plant selfed may give a slightly non-fertile daughter.

Numerous combinations that should be possible in F_2 are omitted in the population obtained. Combinations approaching *N. rustica* seem to be more frequent than those approaching *N. paniculata.* Many more homozygous combinations occur in F_2 than might be expected.

Perfectly fertile plants giving perfectly fertile progeny, heterozygous for many allelomorphs, do occur in F_2.

No more than a very general formal interpretation of these facts can be made at present, but assuming that the chromosomes carry the hereditary character determiners, and that these react with the cytoplasm under proper environmental conditions to build up the soma, attention is called to the following possibilities of satisfying the conditions imposed by the data.

1. There is selective elimination of F_2 zygotes.

2. There is no evidence of selective fertilization. (I infer this from the fact that F_1 plants are as fertile *inter se* as in backcrosses.)

3. The selective elimination of non-functional gametes that must occur in F_1 and the recombinations of functional gametes that give different grades of fertility in F_2 cannot be interpreted by a Mendelian factorial notation without subsidiary assumptions, but possibly may be the result of one of the two following hypotheses:

(*A*) Through multipolar spindles, mating of non-homologous chromosome pairs at synapsis, or other mitotic aberrations at the reduction division, the 24 chromosomes characterizing each of the two species may be irregularly distributed at gametogenesis. If some of these irregular gametes may function, the majority of the experimental data are satisfied, but there are reasons which there is not time to consider which make this scheme improbable.

(*B*) On the other hand the facts may be interpreted without assuming irregularities of chromosome distribution if (1) there is a group of chromosomes in each parent that cannot be replaced by chromosomes from the other parent; if (2) there is a group of chromosomes from each parent, a percentage of which may be replaced by chromosomes from the other parent, but where functional perfection of the gametes varies as their constitution approaches that of the parental forms; if (3) there are other chromosomes that have no effect on fertility and therefore can promote recombinations of characters in the progeny of fertile F_2 plants; if (4) a naked male nucleus entering the normal cytoplasm of the egg in the immediate cross can cause changes in the cytoplasm that will affect future reduction divisions; if (5) this abnormally formed cytoplasm is not equitably distributed in the dichotomies of gametogenesis in the F_1 generation; if (6) it follows from (4) and (5) that F_2 zygotes may be formed which are less perfect in their gamete forming mechanism than those of the F_1 generation; and if (7) the heterotypic division of gametogenesis does not necessarily form two cells alike in their viability.

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